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Behavioral Heterogeneity in Economic Institutions: An Experimental Approach

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[1]This is one of his favorite quotes.

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CHAPTER 1

INTRODUCTION

Human societies are regulated by a set of institutions. According to the Handbook of New Institutional Economics, institutions are "the written and unwritten rules, norms and constraints that humans devise to reduce uncertainty and control their environment. These include (i) written rules and agreements that govern contractual relations and corporate governance, (ii) constitutions, laws and rules that govern politics, government, finance, and society more broadly, and (iii) unwritten codes of conduct, norms of behavior, and beliefs." (p.1) A fundamental feature distinguishing societies lies in the system of rules established to achieve social goals such as efficiency and equity in the distribution of goods and services.

My dissertation uses the laboratory as a testing ground for the performances of various institutional designs and the influences of heterogeneity on their established properties. It consists of three parts. The first part (Chapter 2 and Chapter 3) studies and compares economic institutions in public economics. The second part (Chapter 4 and Chapter 5) examines the impact of two types of heterogeneity on performances of economic institutions. The third part (Chapter 6) provides an example of how heterogeneity affects the choices of institutions.

1.1 Testing institutions via controlled laboratory experiments

One important purpose of controlled laboratory experiments is to test and compare economic institutions such as trading rules, matching mechanisms, or auction designs. In these laboratory experiments, subjects are placed in an artificial, controlled environment, specifying their preferences, goals, initial endowments, as well as the cost and benefit of their choices. Institutions are communicated to the subjects in the form of systems of rules regulating behavior.

The main reason why the laboratory is well suited for testing institutions is that we can define the rules and the reward structure exactly according to the assumptions of the theory. If we fail to do this in the laboratory, then we cannot hope to control for these factors in a "naturally occurring environment". Moreover, the randomization of subjects to treatment eliminates bias from the endogeneity issue. Given such a controlled environment and a specific group of subjects, if a mechanism fails to perform, we have reasons to doubt its external validity in a less-controlled environment (Fiorina and Plott (1978)). For detailed discussions on experimental methodology and comprehensive surveys on experimental results see Smith (1976), Smith (1982), Smith (1994), Plott (1982), Davis and Holt (1993), Roth (1995), Guala (2005) and Alm (2011).

Take an early study on auctions by Coppinger et al. (1980) as an example. In this study, the authors experimentally compare various kinds of auctions (Dutch, English, first- and second-price sealed-bid auctions). According to the Revenue Equivalence Theorem by Vickrey (1961), all auctions satisfying a set of conditions should yield the same revenue to the seller.[1] Coppinger et al. invited university students as subjects of the experiment to bid for a fictitious good. Individual values of the bid are induced by promising a resale value by cash minus the cost if the bid if a subject wins the bid. Information is presented to the subject in the same way as assumed in theory. The main finding of the experiment is that bidding prices in the first-price sealed-bid auction are significantly higher than other forms and subjects learn quite differently in these auctions. This example illustrates how an experimental laboratory serve as a test bed, or "wind tunnel", to check the robustness of institutions even before they are implemented in the real world.

The first part of my dissertation focuses on this theme. In particular, Chapter 2 and Chapter 3 study the effect of economic institutions in the two main research areas in the field of public economics: tax auditing and peer punishment mechanism in public goods contributions.

Chapter 2 experimentally examines a new auditing rule, the bounded rule, which is a better representation of an actual audit selection procedure. The basic setting follows a classic tax-compliance game in which each taxpayer receives either high or low income with certain probabilities. The traditional rule audits every low-income report with a constant probability. The bounded rule audits a randomly selected sample of low-income reports whenever the number of these reports exceeds the maximum number of audits allowed by the budget, or otherwise all of the low-income reports. The experimental evidence suggests that the bounded rule deters potential tax cheaters more cost-effectively.

Chapter 3 investigates the effect of peer punishment on promoting cooperation in the presence of a third-party judge. In a naturally occurring environment, the right

[1]These conditions include: (i) the bidder with the highest type/signal/value always wins, (ii) the bidder with the lowest possible type/value/signal expects zero surplus, (iii) all bidders are risk neutral and (iv) all bidders are drawn from a strictly increasing distribution.

to propose sanctions is often separated from the right to implement sanctions. For instance, when a car driver hurts a cyclist, the latter may sue the former and claim compensation for the damage. It is a judge who ultimately decides whether the punishment request is to be implemented. We design an experiment based on a prisoner's dilemma game to study the effectiveness of punishment in this situation. Players' punishment decisions will be implemented only if an independent third player approves the proposal. We find that both cooperation rate and earnings are significantly lower when a third party decides whether the players' punishment decision, if any, should be implemented. The reason is that both proposed and implemented punishment on defectors is lower when the third party has the final decision on the punishment implementation. Although the intervention of the third party also decreases anti-social punishment, overall it reduces the effectiveness of punishment in promotion cooperation.

1.2 Heterogeneous behavior in the lab

One typical pattern in almost all experiments is that individual decisions exhibit a considerable degree of heterogeneity. For instance, in Chapter 2, around 60 percent of the subjects do not behave in accordance with theory prediction. That is, they switch between honestly reporting and underreporting their income with various levels of frequency. As a result, theory under-predicts the deterrence power of auditing mechanisms.

There are many possible explanations for behavioral differences across individuals. They differ in cognitive ability in understanding a mechanism, or they vary in monetary incentives, or they have diverse interpretations on the "socially appropriate" behavior in a particular situation. In Chapter 3, for example, third-party judges need to make a decision whether to uphold punishment proposals from cooperators on defectors. The result is that responses of the subjects vary substantially. As a result, the actual performance of an economic institution often deviates from the theoretical prediction, which often assumes that players are homogeneous. Understanding how a specific kind of heterogeneity triggers systematic behavioral differences helps to increase the predictable power of economic models and permits an investigation of the robustness of an institution.

The second part of the dissertation aims at addressing how heterogeneity affects the performances of institutions predicted by standard game theory. In order to have tight control over the environment, I induce exogenous heterogeneity among subjects and compare their behavior under identical interaction rules. Doing so facilitates an understanding of the conditions under which the current properties of an institution break down.

One source of behavioral heterogeneity I investigate is being a group or an individual. In the business world, many decisions are made by groups such as boards of directors or management teams. However, much of the economic theory does not distinguish between the two types.

Chapter 4 investigates how group decision makers differ from individual decision makers in terms of market performance. In particular, it compares the behavior of individuals and groups in a repeated sequential Stackelberg market game. Previous experimental literature on intergroup-interindividual decision making suggests that groups are more selfish than individuals. That means in a sequential two-player Stackelberg game, group decisions are predicted to be closer to the subgame-perfect equilibrium than those of individuals.

However, we find that the behavior of groups is farther away from the subgame-perfect equilibrium of the stage game than that of individuals. To a large extent, this result is independent of the method of eliciting choices (truly sequential play or strategy method) and the model used to account for the observed first- and second-mover behavior. The reason lies in the aggregation of individual preferences among group members. In a repeated sequential game, individuals proposing punishment on "greedy" first movers and reward on "nice" ones wins the debate in group discussions. Therefore, collective decisions are polarized in a different direction compared to one-shot games, which was the exclusive focus of the literature. Although the difference between groups and individuals are to the opposite of what previous literature expects, it still implies that ignoring decisions made by groups or individuals is likely to decrease the prediction power of an economic model under a specific set of interaction rules.

In public goods provision, an important source of heterogeneity lies in the abilities of individuals or the cost of cooperation. Take an example of building some street lamps in a neighborhood. As households differ in wealth or the need to use these street lamps, they have different incentives to invest in building such a public good. Think of another example where a group of individuals that must complete a project for which all group members will receive equal credit. However, the effort of some group members, because of higher productivity in the required task, yields greater benefits for all than the same effort from other members. In both cases, the marginal cost or benefits from contribution differ across households or individuals, making it more difficult to reach consensus on the appropriate level of contribution.

Chapter 5 examines the extent to which a punishment mechanism functions when group members have heterogeneous marginal cost and benefit of contribution. In a public goods game setting, I vary the marginal per capita return (MPCR) among group members, so that there are two high and two low productivity players. A well established finding in the experimental public goods literature is that a decentralized, peer punishment mechanism increases cooperation (e.g. Ostrom et al. (1992), Fehr and Gächter (2000), Fehr and Gächter (2002), Masclet et al. (2003), Sefton et al. (2007)). However, comparatively little attention has been paid to scenarios in which agents have asymmetric impacts of contributions on group welfare.

Experimental results indicate that in the absence of sanctions, productivity heterogeneity hampers cooperation. Allowing punishment in these groups significantly enhances the average contributions of group members, but does not increase welfare. In groups in which cooperation is highly successful, high-productivity agents actively punish low-productivity agents in initial periods. However, conditional on individual contributions, high-productivity agents receive more punishment, and behave more responsively by raising their contributions in the next period.

1.3 The effect of heterogeneity on institutional choices

In reality, economic institutions are neither manna from heaven nor exogenously imposed by a fictitious Walrasian auctioneer. In contrast, members of a society often endogenously select the (explicit or implicit) rules that govern their interactions in social and economic exchanges. Examples include the World Trade Organization (WTO) and United Nations Framework Convention on Climate Change (i.e. Kyoto-protocol). Members of these organizations agree upon a series of rules of conduct. If a member later catches another violating certain rules, it could file a suit against the rule violators and demand sanction. Endogenously elected institutions also exist in many areas of daily life. For instance, households in a neighborhood gather together and agree upon the maintenance of their own gardens. If a neighbor thinks one's garden is not being taken good care of, s/he could directly complain to the neighbors, or even ask the community to intervene.

If all individuals are homogenous and their preferences for cooperation are perfectly aligned, it might be much less of a problem to agree upon the same institutional rule. For instance, Ertan et al. (2009) show that effective sanction institutions emerge from a simple voting process when individuals are symmetric.

However, this may not be the case if players are heterogeneous. If an institution is enacted to facilitate the enforcement of a norm, the heterogeneous structure of the environment may lead to conflict in the voting process and thus to inefficient outcomes. This may make it more difficult to achieve consensus on which institution to implement and may lead to a conflict between different types of agent. Such conflicts may prove sustained and durable, with adverse long-term effects on efficiency.

Chapter 6 shows an example of how heterogeneity could lead to suboptimal institutional choice. More specifically, this chapter investigates the effect of heterogeneity on institution selection via a voluntary contributions game. Players may punish others after contributions are made and observed. Following Chapter 5, we induce heterogeneous productivity of contributions by varying the marginal-per-capita return among individuals: two high and two low productivity players. Every two or eight periods, depending on the treatment, individuals vote on a punishment regime, in which certain individuals are permitted, but not required, to have punishment directed toward them. The punishment system can be conditional on type and contribution history. The data indicate that the most effective regime, in terms of contributions and earnings, is one that allows punishment of low contributors only, regardless of productivity. Nevertheless, only a minority of groups converge to this system. The result is due to self-defensive voting: subjects attempt to shut down the punishment channels that might target themselves.

The final chapter of this dissertation is a brief conclusion. It summarizes key lessons from each chapter and discusses future research questions.

CHAPTER 2

DETERRENCE EFFECT OF AUDITING RULES [1]

2.1 Introduction

Tax evasion is a central research topic in public economics. To combat tax evasion, researchers have studied various auditing mechanisms, in which taxpayers have to pay fines if they are caught evading taxes. The simplest way to model the auditing procedure is to assume that each taxpayer is independently selected for audit with a constant probability. In this paper, we term this the *traditional* rule. Due to its simplicity, the traditional rule been widely assumed and studied in the tax compliance literature (see, for example, Allingham and Sandmo (1972), Yitzhaki (1974), Moser et al. (1995), Zimbelman and Waller (1999), Boylan and Sprinkle (2001), Kim et al. (2005), Kim and Waller (2005), Alm et al. (2009) and Kleven et al. (2010)).

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Albeit simple and easily applicable, the traditional rule has some undesirable features. To begin with, the traditional rule is far from being a realistic description of the actual practice. Most organizations, public and private alike, plan their activities such as auditing according to the committed budget of a period. Once the budget is allocated for a certain purpose, it becomes difficult to be reshuffled during the course of a fiscal year. Hence, if the proportion of the "red-flagged" (suspicious) tax returns reports varies significantly across years, it is difficult for the auditor to maintain a target audit probability. Moreover, given the fact that the auditor has to formulate an auditing strategy given a fixed budget, such a simple random auditing rule may not be an efficient way to use the audit resources of a tax agency.

In addition, most of the current studies focus on a one-to-one interaction between a tax authority and a taxpayer, while neglecting the impact of social interactions of taxpayers on evasion decisions. Recent studies on tax behavior from an economic psychology perspective argue that decisions to comply are affected by personal, social and societal norms (Kirchler (2007)). Personal norm, which is defined as "a moral imperative that one should deliberately comply", is associated with factors such as moral reasoning, religious beliefs and political party preference. Social norm, according to Wenzel (2005), is "prevalence or acceptance of tax evasion among a reference group" (e.g. friends, colleagues or acquaintances). Societal (or cultural) norms, which reflect the general attitude towards tax evasion in a large population, are often termed tax morale or civic duty. In summary, the compliance decisions of taxpayers do not merely depend on their isolated assessments of economic variables such as income, audit probability and fine, but also on their beliefs on what they should do and what others do. Given the limited audit resources of a tax authority for a fixed period of time, the interdependent beliefs of the taxpayers may affect their compliance decisions, and consequently the ex-post probability of being audited. This could lead to distinctive decision dynamics and equilibria across societies.

A recent paper by Yim (2009) analyzes an auditing rule known as the bounded rule to address these undesirable features of the traditional rule. He argues that owing to a budget constraint, a tax authority cannot perform more than some fixed number of audits. Hence, the bounded rule is a variable-rate rule. That means a tax authority should always exhaust all the audit resources, i.e. audit up to the maximum number given the budget constraint if necessary. Because the number of reports selected for audit is bounded by the audit capacity, the audit probability facing a taxpayer varies. A taxpayer has to infer the audit probability by forming expectations on others' decisions. Through this channel, he naturally incorporates the analysis of beliefs via game theory. The main result of Yim (2009) is that given that taxpayers are self-interested utility maximizers, the bounded rule could induce the same level of compliance as the traditional rule.

This paper asks two research questions. First, is the actual compliance under the bounded rule the same as the traditional rule widely studied in the literature? In particular, does the bounded rule trigger more thoughts on others' decisions and hence lead to different levels of compliance? Second, how does the level of strategic uncertainty affect behavior? That is, how will taxpayers react when they are less certain of the actual audit probability faced? Are they more or less likely to think that others will cheat on taxes?

This paper takes an experimental approach to examine the bounded rule empirically. Compared to field data, the laboratory provides tight controls on tax reporting institutions (audit probability, tax rate, and income level). Moreover, it also allows us to measure tax evasion behavior repeatedly and inexpensively without the measurement errors which exist in field data (see the discussions in Torgler (2002)).

Our laboratory setting follows the key features of a classical tax compliance game first developed by Graetz et al. (1986). Every taxpayer has a certain probability of receiving high or low income. Knowing a certain auditing rule, they have to decide simultaneously and independently whether to report their income truthfully to the tax authority. The traditional rule audits every low-income report with a constant probability. The bounded rule audits a randomly selected sample of low- income reports whenever the number of these reports exceeds the maximum number of audits allowed by the budget, or otherwise all of the low-income reports. Depending on the treatment, the tax authority implements either the traditional or the bounded rule after deducting taxes according to players' reported income.

To examine the first question, we select parameters for the bounded rule such that 1) the deterrence effect of the bounded rule in this treatment is the same as that of the traditional rule; and 2) the level of strategic uncertainty is low in that players are more certain about the audit probability. To study the second question, we increase the level of strategic uncertainty among players. The equilibria depend on the independent beliefs of the taxpayers. If they are too optimistic (pessimistic) about their underreporting decisions, they will all choose underreporting (honestly reporting) in equilibrium.

The main results of our experiment are the following. Consistent with the theoretical predictions, the bounded rule induces the same level of tax compliance as the traditional rule. When the level of strategic uncertainty increases, the bounded rule becomes even more effective in deterring tax evaders even though the maximum number of audits of a tax authority does not change. The data also show that theory over-predicts the level of evasion. In other words, the compliance level under both rules is higher than theoretical predictions. To explain this, we develop a bounded rational model where taxpayers play the more profitable strategy with higher probabilities. We find that behavior in our data is consistent with loss aversion combined with random decision errors.

Our paper makes the following contributions to the tax compliance literature. To begin with, this study is the first empirical examination of the bounded rule, which explicitly models interactions among taxpayers and therefore offers the possibility of modeling the effect of norms on tax evasion decisions in the language of economics (i.e. game theory). By comparing the level of compliance induced by the bounded rule and a well studied flat-rate rule in a controlled laboratory experiment, we set the stage for using this rule to examine the effect of norms on taxpayers' decisions in the future.

In addition, experimental data suggest that uncertainty from other taxpayers' actions could be a source leading to the "tax-compliance" puzzle. That is, even though taxpayers may be aware of the limited audit capacity of a tax authority, the uncertainty of others' decisions makes it difficult to access the actual audit probability. This is particularly so when the degree of uncertainty is high. From the policy viewpoint, strategic uncertainty could be used as a very good resource to deter tax evasion.

The chapter is organized as follows. Section 2.2 summarizes studies aiming at developing and testing non-random auditing rules in the literature. Section 2.3 describes the tax-compliance model and auditing rules that are examined in the experiment. Section 2.4 constructs an experimental design and presents the testing hypotheses. Section 2.5 analyses the experimental data with both nonparametric and parametric methods. Finally, Section 2.7 concludes and discusses directions for future research.

2.2 Conditional audit mechanisms

The traditional rule and its variants are widely studied in the literature (see the literature review by Andreoni et al. (1998), Alm and McKee (1998) and Slemrod and Yitzhaki (2002)). A meta study by Blackwell (2007) based on twenty laboratory experimental studies finds that an increase in audit probability or fine rate leads to higher compliance, but the tax rate has no significant effect.

Some papers argue that an efficient way to deter tax cheaters is by letting auditing probabilities depend on history (Harrington (1988), Landsberger and Meilijson (1982) and Greenberg (1984)). If taxpayers are caught to be non-compliant in the current period, their future audit probability and fines are increased. Friesen (2003) proposes an alternative mechanism where the fine and audit probability decreases when taxpayers are compliant in the current period. Friesen argues this mechanism reduces auditing costs compared with the strategy suggested by Harrington (1988).

Cason and Gangadharan (2006) experimentally test the Harrington (1988) model by assigning a higher (lower) auditing probability and a more severe fine to taxpayers who are detected evading (complying with) taxes. The model predicts that taxpayers with lower compliance cost should not evade taxes in this setting. The data qualitatively support this finding. Clark et al. (2004) conduct an experiment to compare two specific schemes - Harrington's past-compliance rule (Harrington (1988)) and Friesen's optimal rule (Friesen (2003)) – against the random auditing rule. The data suggest that the random auditing rule generally deters tax evaders effectively, although more audits are needed compared to the optimal rule. The past-compliance rule is intermediate both in terms of deterrence and cost of audit.

Another strand of literature lets audit probability depend on reported income. Reinganum and Wilde (1985) analyze an "audit cutoff" policy in which an audit is triggered if the reported income is below a certain threshold, and otherwise no audit if the reported income is above the threshold. They show that there exists an equilibrium where the audit probability is decreasing in the level of reported income; and that all taxpayers underreport, although by an amount which decreases in true income. Follow-up papers conclude that if the audit probability could depend on reported income, the optimal strategy for the auditor is to randomly audit individuals who report below some threshold level of income. In equilibrium, only low-income taxpayers report honestly, while high-income taxpayers report exactly at the threshold level (Sanchez and Sobel (1993), Cremer and Gahvari (1996), Mookherjee and Png (1989), Scotchmer (1987) , Bayer and Cowell (2009)).

The above models require the auditor to announce and commit to an audit policy before receiving taxpayers' reports. Many later papers assume that the auditor is also a strategic player who chooses an audit probability based on reported income. Graetz et al. (1986) consider a model with two levels of true income for taxpayers and an unlimited budget for the auditor. They characterize a unique mixed-strategy Nash equilibrium for the interaction between taxpayers and the auditor. Slemrod and Yitzhaki (2002) provide detailed discussion on these alternative auditing rules.

Alm et al. (1993) experimentally compare the effectiveness of several auditing rules: a purely random rule; a forward-looking rule which increases future auditing probabilities of current tax evaders; a backward-looking rule which triggers audits of taxpayers if they are caught cheating on taxes in the current period; and a cut-off rule combining a sure audit below a threshold on reported income and a small, random audit above the threshold. They find that the cut-off rule is the most effective in deterring tax evaders. Unlike the bounded rule, however, the cut-off requires a large number of random audits. Apart from that, the endogenous auditing rules in their paper implicitly assume that the tax auditor does not face a budget constraint, as the overall number of audits is based on the decisions of the taxpayers.

In all the above studies, the tax evasion game still focuses on the interaction between the auditor and a taxpayer, without inducing interactions among taxpayers. Alm and McKee (2004) experimentally study a cut-off rule with audit probability depending on a cut-off threshold as well as the deviation of an individual's reported income from the average of the incomes reported by all other players. This mechanism induces a coordination problem for taxpayers who want to cheat on taxes. They include treatments with and without pre-play communication. The main finding is that pre-play communication helps players to strategically coordinate on reporting the threshold income. Adding some random audits on the threshold into the original mechanism helps to overcome the player coordination problem. Our paper differs from theirs in that taxpayers within a group do not always receive the same level of income in a given period. Furthermore, the interaction induced by the bounded rule among taxpayers does not always need to be a coordination game.

Gilpatric et al. (2011) study two endogenous audit mechanisms both analytically and experimentally. The tournament audit mechanism audits K out of N players for which the difference between the expected and actual production reports is the largest. The generalized relative evaluation (GRE) mechanism audits a firm based on how much lower the reported output is compared to other peers in the same group. They find that compliance levels are similar between the two endogenous schemes but are both higher than a simple random audit. Compared to their study, our simpler setup with equally suspicious low-income reports allows us to clearly examine the effect of audit capacity on the interactions among taxpayers. With this setup, one can understand whether the effects of the tournament / generalized relative evaluation mechanisms studied by Gilpatric et al. (2011) are driven by the relative evaluation feature of the mechanisms, or maybe other effects of an audit rule without relative evaluation but with an audit capacity. Moreover, due to different setups of the models, the game of our study could be a dominance solvable game or a coordination game instead of a game with a unique symmetric equilibrium in their setting. If the audit capacity that characterizes the bounded rule does indeed capture a salient feature of the reality, then maybe a tax authority with its enforcement strength restrained by the audit capacity also faces the uncertainty of ending up in the "bad" equilibrium of a multiple-equilibrium game. It is an empirical issue to verify whether people can tacitly coordinate to play the equilibrium undesirable to the tax authority. Our study provides some evidence to answer the question.

The auditing rule proposed by Yim (2009) is the closest to our study. In this model, a tax auditor first selects a committed budget to set aside resources that may be used to support audits in the current period, given the income distribution of the taxpayers. Knowing the budget selected by the tax auditor, the taxpayers simultaneously decide to report their income. Then the auditor implements a certain auditing rule to catch tax evaders. Yim analytically demonstrates that the bounded rule is the most efficient auditing rule, which demands the lowest committed budget necessary to maintain a given level of deterrence.

A key assumption distinguishing this paper from Yim (2009) is the ability of the auditor to commit to an auditing strategy. In Yim (2009), the auditor interacts strategically with taxpayers, and chooses the audit probability on observing the behavior of taxpayers. In theory, we can construct the two auditing rules such that the induced compliance level is the same when auditors and taxpayers play Nash equilibrium in both games. Nevertheless, this requires a demanding understanding of the game and mutual belief in each others' actions. Any off-equilibrium decisions by the auditors will lead to the behavior of the taxpayers being incomparable under the two rules. In this study, we let the auditor commit to an auditing strategy so that the focus is on the reactions of the taxpayers. Consequently, this paper should not be considered as a direct test of Yim's model, as the properties of the bounded rule explored in this study are fundamentally different. Instead, it focuses on examining whether and how human subjects react to the two rules.

2.3 Model description

The model of this paper builds upon Yim (2009), which follows some basic features of a classic tax-compliance game developed by Graetz et al. (1986). Consider a player population of size N. For simplicity, the model assumes two income classes: high and low, denoted I_H and I_L, respectively, where $I_L < I_H$. Each player has a probability q of being a high-income taxpayer (H-type) and $1-q$ of being a low-income taxpayer (L-type), where $0 < q < 1$. Players know the type distribution as well as their own types, but they do not know the exact types of the other players. Each player has to decide simultaneously and privately whether to report high income (I_H) or low income (I_L) to the tax authority. Let T_H and T_L for the tax payment by high- and low-income taxpayers, respectively, where $T_H < I_H$, $T_L < I_L$, and $T_L < T_H$. If cheaters are audited, then a fine F is imposed on top of the tax they should have paid ($F > 0$). However, taxpayers who report truthfully are never fined and incur no cost if they are audited. The following analysis assumes that players are homogeneous, rational, risk-neutral profit maximizers.

The *traditional rule* can be presented easily. Any taxpayer who has filed a "low-income" report will face a flat probability a_{TR} of being audited independently. Since reporting truthfully does not incur any cost when being audited, L-type players always report their income truthfully. If they report high income, they will be taxed T_H, which is strictly larger than the tax T_L they need to pay if they honestly state income. For H-type players, the honest-reporting payoff is $I_H - T_H$. If they underreport, the payoff is $I_H - T_L$ if they are not audited, and $I_H - T_H - F$ if they are audited. Therefore, they choose to underreport if and only if the expected profit is strictly larger:

$$(1 - a_{TR})(I_H - T_L) + a_{TR}(I_H - T_H - F) > (I_H - T_H).$$

If the audit probability is less than the threshold $\bar{a}$ defined by

$$\bar{a} = (T_H - T_L)/(F + T_H - T_L),$$

the H-type players will underreport. Otherwise, if the audit probability is larger than $\bar{a}$, they choose to report truthfully.

The *bounded rule* is characterized by the maximum number of K audits allowed, given a budget. It then constructs an audit sample size contingent on the number of "low-income" reports L. If L is smaller than or equal to the audit capacity K, the auditor will audit all L reports. However, if L is strictly larger than K, then the auditor will randomly audit K reports. Expressed more formally, every " low-income" taxpayer under the bounded rule faces the following audit probability:

$$a_{BD} = \begin{cases} 1 & \text{if} \quad L \leq K \\ K/L & \text{if} \quad L > K \end{cases}$$

for $L = 0, 1, ...N$.

The key feature of the bounded rule is that the audit probability a_{BD} is no longer exogenously given. Instead, it depends on the audit capacity K and the number of reported "low-income" files L. The latter is a function of population size N and the ex-ante probability q being an H-type. The following proposition characterizes a property of the bounded rule.

Proposition 2.1 *For any given N and q, the auditor can always choose an audit capacity K for the bounded rule such that it induces the same compliance level as the traditional rule.*

Proof: See appendix 2.A. ∎

The intuition of Proposition 2.1 is as follows. Any audit probability a_{TR} under the traditional rule induces all-or-none compliance behavior. If the maximum number of K is so high that all "low-income" reports will always be audited, H-type players will have no incentive to underreport. On the other hand, if K is zero (meaning that no audit is conducted regardless of the number of "low-income" reports submitted), then H-type players will underreport with certainty. Between these two extreme cases there exists a threshold $\bar{K}$ such that any $K > \bar{K}$ sustains compliance behavior regardless of the actual income-realization parameter q. That is, even in the scenario where all taxpayers claim low income, the audit probability is still high enough to deter tax evasion.

To induce full compliance, however, the committed budget K does not always need to be larger than $\bar{K}$. Put differently, even when $K < \bar{K}$, the bounded rule is still able to induce full compliance. Depending on the parameters, the interactions among taxpayers induced by the bounded rule could either be a dominance-solvable game with one unique equilibrium, or a coordination game with multiple equilibria. We construct two treatments to study the performances of the bounded rule under each situation.

2.4 The experiment

2.4.1 Design

The tax-compliance game in all treatments has three stages: (i) income reporting and tax deduction, (ii) audit and fine deduction, and (iii) feedback. Subjects receive either

Table 2.1: Experimental treatments

Treatment	High-income probability q	Audit probability a or capacity k	Number of subjects	Number of sessions
Baseline	0.5	$a = 0.4$	64	4
Bounded	0.5	$K = 2$	64	4
Bounded-Hq	0.9	$K = 2$	64	4

high income (I_H) €25 or low income (I_L) €10 with probability (q) 0.5. Subjects are informed about the group size N and the probability q. Based on the capacity constraint in the lab, the size of the taxpayer population is fixed to be $N = 8$. The parameter q is either 0.5 or 0.9 depending on the treatment. During the income-reporting stage, they have to decide simultaneously and independently the type of income to report to an auditor, which is simulated by a computer. The computer automatically deducts taxes according to the reported income. The tax for subjects reporting "high income" (T_H) is €12.5, whereas the tax for subjects reporting "low income" (T_L) is €2.5.[2] Subjects are told that taxes are deducted based on their reported income instead of true income. For instance, H-type players receive €22.5, instead of €12.5, if they submit "low-income" reports. Similarly, L-type players receive -€2.5, instead of €7.5, if they submit "high-income" reports.[3] In the audit stage, the computer implements either a traditional rule or a bounded rule to audit "low-income" taxpayers.

Table 2.1 summarizes the treatment design.

Traditional: In this treatment, subjects filing "low-income" reports face an independent audit probability of 0.4. This audit probability induces the same compliance rate to the bounded rule.[4] If they report honestly, nothing will happen to their final payoffs. However, if cheaters are caught by the auditor, then they need to pay back the €10 of taxes evaded plus a fine (F) of €10.

Bounded: In this treatment, the audit probability depends on the total number of " low-income" reports received. The maximum number of audits to be conducted is $K = 2$. This means that if the number of low-income reports does not exceed two,

[2]Experimental parameters concerning taxation are chosen to be in line with reality. For instance, the real-world tax rates for high-income and low-income taxpayers are usually dependent on the levels of their incomes. In particular, many countries such as Britain, the Netherlands, Germany, Italy and the USA use a progressive tax system instead of a proportional one. Hence, this experiment adopts a progressive tax system for the sake of facilitating subjects' understanding.

[3]Even when a subject with low income makes a loss by submitting "high income" reports and that decision is selected for payment, the potential loss is covered by a show-up fee of €3. During the experiment sessions, this situation never actually happens.

[4]Due to the fact that the traditional rule induces all-or-none behavior in compliance, any audit probability $a < 0.5$ is theoretically equivalent to the bounded rule. Nevertheless, this statement only holds for perfectly rational, risk-neutral players. To what extent this holds for risk averse players is an empirical question to be tested.

then all of them will be audited with probability 1. Otherwise, the audit probability decreases monotonically with the number of "low-income" reports L. In particular, the probability is 0.67 for $L = 3$; 0.5 for $L = 4$; 0.4 for $L = 5$; 0.33 for $L = 6$; 0.29 for $L = 7$; and 0.25 for $L = 8$. This parameter K guarantees a unique Nash equilibrium based on non-cooperative game theory (see analysis below). The fine for cheaters is exactly the same as in the *Traditional* treatment.

Bounded-Hq: Everything in this treatment remains the same as the *Bounded* treatment, except that the ex-ante probability of receiving high-income q becomes 0.9 instead of 0.5. A high q roughly resembles a situation where each household in a rich neighborhood is likely to be wealthy. Compared to the *Bounded* treatment, subjects in this treatment face a higher degree of uncertainty, since fewer taxpayers will certainly submit "low-income" reports. In other words, the fewer L-type players in the population who honestly state their type with certainty, the more difficult for the H-type players to pretend to be L-type. We are interested in knowing whether the bounded rule loses its deterrence effect in the presence of multiple equilibria.

Admittedly, for each auditing probability in the traditional rule a, there exists more than one set of parameters N, K, q that trigger the same level of deterrence in theory. We select $N = 8$ based on the capacity of a conventional laboratory. Given $N = 8$, setting $K = 2$ gives us the possibility to examine the various properties of the bounded rule with different parameters qs. To maximize the salience while not to the extreme of $q = 1$ that all taxpayers in the experiment are surely H-income taxpayers, we believe $q = 0.9$ strikes the best balance in this consideration.

2.4.2 Procedures

The experiments are conducted at the CentER Lab in Tilburg University from October to December 2009. Tilburg University students, mostly majoring in economics or business, participate as subjects in the experiment. Each treatment consists of four sessions of 16 subjects each. The duration of a session is about 1 hour (including the initial instruction and final payment to subjects). The average earnings are €16.23 (including the €3 show-up fee). The experiments are programmed and conducted in Z-Tree software (Fischbacher (2007)).

At the beginning of each session, subjects are randomly assigned to the computer terminals. Before the experiment starts, subjects have to complete an exercise to make sure they understand the rules of the game.

The game consists of 30 periods. At the beginning of each period, 16 subjects are randomly allocated into two groups of eight. The random re-matching protocol minimizes the chances that subjects encounter the same group of participants again. It simulates a one-shot scenario but allows the subjects to be familiar with the game environment. At the end of each period, a summary screen is presented to subjects with feedback information including the subject's true and reported income, and the final payoff for the period. Subjects are not informed of others' payoffs.

Upon completing the tax-compliance experiment, subjects are asked to complete a risk elicitation task similar to the one used by Holt and Laury (2002).The instructions for the risk elicitation task are handed out only after the tax-compliance game. Hence, the subjects are not aware of its existence beforehand. In this task, subjects have to make selections of a set of 21 lottery pairs. Each lottery pair consists of a safe and a risky lottery. The expected payoff of the risky lottery compared to the safe one is the lowest in the first pair, and the highest in the last pair. The switching point from the safe to the risky lottery reflects subjects' risk tolerance level. These data are used to explain behavior in the tax-compliance game.

At the end of the experiment, subjects are asked to complete two questionnaires. The first one concerns social background information such as gender, nationality, and years of studying economics. The second one elicits subjects' ethical orientation by the Machiavellian IV scale personality test (see Christie and Geis (1970)).[5]

During the payment stage, one period of the tax game and the realization of one lottery are randomly selected to determine the final payment of a subject. This random payment scheme mitigates the potential income effect that subjects carry across games and over different periods within a game.

2.4.3 Theoretical predictions

To derive testable hypotheses, we start by assuming that players are self-interested profit maximizers. One could argue that some taxpayers are inequality-averse and hence attempt influence income distributions in the population. In this setting, however, players do not know the actual realization of the others' income types. The incomplete information makes it difficult for the players to equalize payoffs of the others. To simplify the analysis, we stick to the homoeconomicus assumption. We then discuss how personal and social norms affect the robustness of predictions.

In this study, the deterrence effect is indicated by the underreporting rate in the population: namely, the proportion of high-income taxpayers filing "low-income" reports in a certain period. As discussed in Section 2.3, the analysis focuses on the H-type players, as the L-type players have a dominant strategy of reporting honestly, regardless of the auditing rules.[6] In the following, let h be the honestly reporting strategy for H-type players, and u be the underreporting strategy.

Traditional: As the audit probability a_{TR} is set to be 0.4, an underreporting decision is equivalent to selecting a lottery of €22.5 with probability 0.6 and €2.5 with probability 0.4. The expected payoff is therefore: $E(\pi_u) = €22.5 \times 0.6 + €2.5 \times 0.4 = €14.5$.

[5]Generally, this test measures a person's predisposition to act in accordance with one's own interests over ethical standards. A higher score indicates that a person is more individualistic and loosely bound to conventional moral standards.

[6]The actual percentages of honest reports among L-type taxpayers are 99.68% and 99.28% across treatments, suggesting that they do play the dominant strategy.

As it is strictly larger than the certain payoff €12.5 from an honest report, H-type players are expected to underreport.

Bounded: The H-type players again face the tax-evasion gamble of choosing a certain payoff of €12.5, or a high payoff of €22.5 if they are not audited but a low payoff €2.5 otherwise. Unlike the traditional rule, however, the audit probability a_{BD} is not exogenously given. Instead, it depends on the players' perception of the actions of others. In particular, it depends on player i's subjective belief on the likelihood of the proportion of "low-income" reports turned in by another player, denoted by B_i.

A "low-income" report could come from two sources. The first source is from a truth-telling L-type player with probability $1 - q$. Alternatively, it could come from H-type players who dishonestly report that they have received low income. If a player thinks that the underreporting probability of H-type players i is b_i, this scenario will occur with probability qb_i. Hence, the overall probability of observing a "low-income" report B_i for player i is the sum of the probabilities in these two situations: $B_i = 1 - q + qb_i$.

The Nash equilibrium in this treatment can be reached by iterated elimination of dominated strategies. The intuition proceeds as follows. Reporting high income is a dominated strategy for L-type players, since they have to pay a high tax and incur a lower payoff than they would otherwise. If the H-type players believe that the L-type obey dominance, then the strategy of reporting truthfully (h) is dominated. That is, even when an H-type player believes that no other players evade taxes, the expected payoff of underreporting is still higher than that of honest reporting. Such a high expected payoff is caused by a low audit probability strictly less than 0.5, which stems from the fact that all of the L-type players (about half of the population) state low income truthfully. The calculation also guarantees that evading taxes is always the best response for an H-type player when L-type players obey dominance. Proposition 2.2 derives the equilibrium underreporting decisions.

Proposition 2.2 *Given the set of parameters* $(N, K, q) = (8, 2, 0.5)$, *the game introduced by the bounded rule is dominance solvable. In the equilibrium, both the L-type and H-type players report "low income".*

Proof: See appendix 2.A. ∎

Bounded-Hq: According to non-cooperative game theory, the introduction of the bounded rule with the same audit capacity changes the interaction of players into a coordination game with incomplete information. We ignore other asymmetric equilibria in a symmetric setting, as these equilibria require unrealistic coordination among symmetric players.

Proposition 2.3 *Given the set of parameters* $(N, K, q) = (8, 2, 0.9)$*, the game has two pure-strategy Nash equilibria and one mixed-strategy Nash equilibrium. In the pure-strategy equilibria, L-type players play their dominant strategy of reporting truthfully. All H-type players opt for underreporting (truth-reporting) if they believe other H-type players are going to cheat with probability higher(lower) than* 0.432*. There is also a symmetric mixed-strategy Nash equilibrium, in which H-type players underreport with probability* 0.432.

Proof: See appendix 2.A. ∎

The analysis so far assumes that taxpayers are all homo-economicus, self-interested profit maximizers. However, field studies categorised taxpayers as "typical taxpayers", "honest taxpayers" or "tax evaders" based on their attitudes towards tax evasion (see Kirchler (1998)). Even in controlled laboratory experiments with low stakes and punishment, many studies still find a considerable number of subjects who constantly behave honestly (e.g. James and Alley (2002)). Recent economic-psychology research on tax behavior has focused on the impact of norms on compliance. In particular, we consider two types of norms that may affect taxpayers' decisions. The first type is the personal norm, which is defined as "a moral imperative that one should deliberately comply" (Kirchler (2007), p59). The sources of personal norm, or tax ethics, could be moral reasoning (e.g. Trivedi et al. (2003), Kirchler (1998)), strong religious beliefs (e.g. Torgler (2003)) and political party preference (e.g. Wahlund (1992)).

How does the presence of some honest taxpayers affect predictions if they always report income truthfully? It turns out that it does not change the direction in terms of treatment differences. Recall that in the *Bounded* treatment, the optimal strategy of the H-type players does not depend on their beliefs towards other H-type players. As long as they believe that L-types will not play the dominated strategy (i.e. reporting high income), they can form expectations on the proportion of "low-income" reports filed in each realized income distribution. Given that the ex-ante probability of being an L-type player is sufficiently high ($q = 0.5$), the certain payoff for an H-type player to report honestly is lower than the expected payoff from underreporting, even when s/he does not expect any other H-types to underreport. This ensures that all H-type players will continue to underreport with or without honest players. The analysis in the *Traditional* treatment is simpler. As player decisions are independent, the audit probability facing self-regarding profit maximizers is unaffected by honest players. In sum, if the percentage of intrinsically honest players is assumed to be the same in both treatments, the compliance rate is the same.

This analysis for the *Bounded-Hq* treatment is a bit more complicated. Let each taxpayer have a probability ρ of being an honest player. If ρ is sufficiently large, strategic players will find underreporting too risky to be worth the attempt. If that is the case, this modification could be considered as a refinement of the coordination game. However, if ρ is small, the payoff-dominant Nash equilibrium still exists, if a strategic player has a strong belief in the noncompliance behavior of the other strategic players. We find intrinsically honest players consist of 15% of all subjects in the *Traditional* treatment. Assuming strategic players correctly anticipate that $\rho = 0.15$, the threshold beliefs inducing underreporting behavior increases to 0.508. Nonetheless, the two pure-strategy equilibria remain the same.

The second type of norm is the social norm, which according to Wenzel (2005), is "prevalence or acceptance of tax evasion among a reference group". That means a taxpayer could be "conditionally honest". If he believes that non-compliance is widespread and a socially accepted behavior, then s/he is less likely to comply.

Assume that some players are affected by the social norm. In the *Traditional* treatment, players do not have the opportunity to communicate or to infer the evasion decisions of the others. Thus, the degree of compliance in the population will depend on the proportion of taxpayers who think that others will honestly report taxes. Intuitively speaking, if the expectations of players are rational, the more self-interested taxpayers who maximize their expected profit by underreporting their income in the population, the less likely it is that "conditionally honest taxpayers will act honestly as well.

Now turn to the *Bounded* treatment. Regardless of the existence of conditionally honest taxpayers, self-interested taxpayers will underreport. Similarly to the *Traditional* treatment, the more self-interested taxpayers in the population, the less "conditional honest" taxpayers will comply. If the distribution of two types of taxpayers is the same across treatments (as the subjects are randomly selected from one large population), the presence of "conditional honest" taxpayers should have the same impact on the level of compliance in both treatments.

Let b^{TR} be the underreporting rate in the *Traditional* treatment, and b^{BD} be the underreporting rate in the *Bounded* treatment. The hypothesis is built upon Proposition 2.2:

Hypothesis 2.1 *Given the set of parameters $(N, K, q) = (8, 2, 0.5)$, the underreporting rates are the same under both rules: $b^{TR} = b^{BD}$.*

Although the predictions of the *Traditional* and the *Bounded* follow the same direction, this is less clear for the *Bounded-Hq* treatment. In the presence of multi-equilibria, the beliefs and strategies of the self-interested taxpayers and conditional honest taxpayers are inter-dependent. Although theory does not predict which equilibrium players will select, experimental papers on coordination games might point out a direction. Numerous laboratory studies on order-statistic coordination games (e.g. Huyck et al. (1990), Huyck et al. (1991), Blume and Ortmann (2007), Chaudhuri et al. (2005)) and stag-hunt games (e.g. Cooper et al. (1990). Cooper et al. (1992)) have reported the attractiveness of a secure strategy. That is, players fail to coordinate on the payoff dominant equilibrium in those experiments. The robustness of this result seems to relate to a series of factors in game structures such as group size and the relative payoff attractiveness of the equilibria, as well as behavioral determinants such as initial choices and pre-play communication.[7] Since the game structure and design features in our *Bounded-Hq* treatment are similar to the coordination games tested in the previous experiments, we expect a stronger attraction for the risk-dominant equilibrium. That is, we hypothesize a higher tendency for subjects to honestly report their income in the *Bound-Hq* treatment than in the *Bounded* treatment.

Let b^{HQ} be the underreporting rate in the *Bounded-Hq* treatment.

Hypothesis 2.2 *Given the set of parameters* $(N, K, q) = (8, 2, 0.9)$*, the underreporting rate in the* Bounded-Hq *treatment is lower than that of the* Bounded *treatment:* $b^{HQ} < b^{BD}$.

2.5 Results

Treatment effects

This subsection focuses on the comparisons of underreporting rates across treatments. Table 2.2 summarizes the descriptive results of non-compliance behavior and profits across experimental treatments. Columns 2 to 4 contain averages over all 30 periods of play, and columns 5 to 7 contain the results for the last 10 periods, where the behavioral pattern is more stable. A session is an independent observation, due to the fact that players of eight are rematched for each period in the *Bounded* treatment. All the statistical tests in this section are two-sided Wilcoxon rank-sum tests.

Results are summarized as follows:

Result 2.1 *Hypothesis 2.1 is supported. The observed underreporting rates are not statistically different between the two treatments.*

Support: The overall underreport frequency is 60.83% in the *Traditional* treatment and 57.11% in the *Bounded* treatment where players receive high income. A two-sided Wilcoxon rank-sum test cannot reject the null hypothesis that the underreport frequencies of the two treatments are the same ($p = 0.386$). In the last 10 periods, the difference across treatments becomes slightly larger, but is still not statistically significant ($p = 0.564$). Note that the statistical power of Result 2.1 is limited due to the fact that each treatment only consists of 4 independent observations. To see whether our conclusion on Hypothesis 1 is robust, we have run a logistic regression using subject-period observations. The dependent variable equals 1 if an H-type subject underreports in a period and 0 if s/he honestly reports in the period. The independent variable is whether the observation comes from the Bounded or Flat-rate treatment (with or without social demographic controls). Based on a standard error corrected for clustering by subject, the estimated coefficient of the treatment variable is negative but statistically insignificant. This further confirms that the underreporting rates in the two treatments are statistically indistinguishable. ∎

[7] For a comprehensive review on the conditions of coordination failure, see Devetag and Ortmann (2007).

Table 2.2: Summary statistics across treatments

	All 30 periods			**Last 10 periods**		
	Traditional	*Bounded*	*Bounded-Hq*	*Traditional*	*Bounded*	*Bounded-Hq*
All subjects						
High-income probability	0.514	0.491	0.898	0.527	0.519	0.908
	(0.007)	(0.039)	(0.024)	(0.042)	(0.038)	(0.013)
Percentage of	79.741%	78.853%	40.312 %	77.969	% 75.935%	32.971%
"low-income" reports	(0.074)	(0.015)	(0.053)	(0.066)	(0.018)	(0.055)
Average earnings	10.675	11.044	11.404	10.321	11.156	11.133
	(0.172)	(0.240)	(5.669)	(0.234)	(0.585)	(4.922)
Revenue ratio	0.921	0.853	1.051	0.999	0.854	1.083
	(0.027)	(0.018)	(0.025)	(0.049)	(0.072)	(0.042)
Traditional v. Bounded	$p<0.05$			$p<0.05$		
Bounded v. Bounded-Hq		$p<0.05$			$p<0.05$	
H-type subjects						
Underreport frequency	60.829%	57.114%	33.951%	58.163%	53.321%	26.160%
(base: H-type)	(0.144)	(0.144)	(0.038)	(0.143)	(0.052)	(0.046)
Traditional v. Bounded	$p=0.386$			$p=0.564$		
Bounded v. Bounded-Hq		$p<0.05$			$p<0.05$	
Cheating frequency	31.303%	28.335%	30.365%	30.625%	27.345%	23.753%
(base: all players)	(0.070)	(0.036)	(0.033)	(0.083)	(0.047)	(0.045)
Traditional v. Bounded	$p=0.685$			$p=0.485$		
Bounded v. Bounded-Hq		$p=0.342$			$p=0.248$	

Notes: Standard errors are in the parentheses. To control for repeated measures, we take the averages of variables over 30/10 periods for each session. We treat each session as an independent observation. All the non-parametric tests reported in this table are two-sided Mann-Whitney rank-sum tests.

Result 2.2 *Hypothesis 2.2 is supported. The non-compliance rate in the* Bounded-Hq *treatment is significantly lower than it is in both the* Traditional *and the* Bounded *treatments.*

Support: In line with what is found in the previous literature on coordination games, subjects in our experiment fail to coordinate on the payoff dominant equilibrium in which they all underreport their income. The overall underreport rate in the *Bounded-Hq* treatment is 33.95% over all 30 periods, and drops to 26.16% in the last 10 periods. The deterrence effect of the bounded rule is the strongest, as the non-compliance frequency is significantly lower compared to the other two treatments ($p < 0.05$). This difference is already salient in the first period, and remains highly significant throughout the game.

There are other interesting results worth discussing. Let us first focus on the *Traditional* and *Bounded* treatments. The three rows on top of the table report statistics concerning all subjects. The first row indicates that the actual probability of being an H-type in both treatments is very close to their pre-specified levels with repeated drawing. The second row displays the percentage of low-income reports among all reports (i.e. reports from L-type players and the untruthful ones by H-type players). The average earnings presented in the third row are not statistically significant ($p = 0.114$). The fourth row reports the revenue ratio of the tax authority across treatments. Revenue ratio is defined as the actual revenue of the auditor (i.e. tax collection and fines from catching cheaters minus audit cost) over the revenue collected if nobody ever cheats on taxes. Since cost per audit is not defined explicitly in the experiment, we treat it as zero. The result indicates that the revenue ratio is the highest in the Bounded-Hq treatment due to a higher efficiency in catching tax cheaters (see audit selection rates).

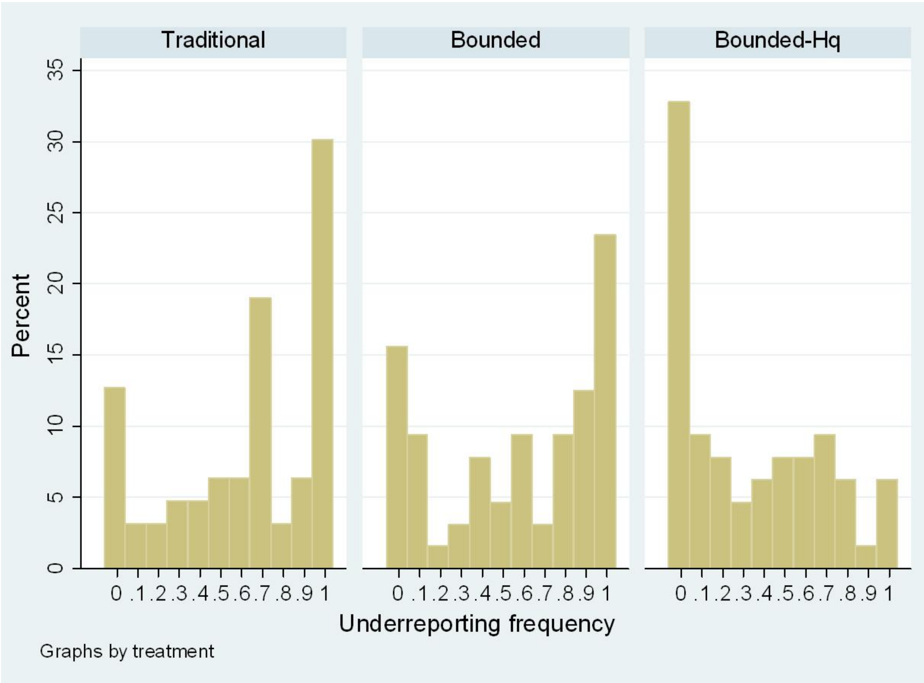
The fact that the revenue ratio is higher in the *Traditional* treatment than in the *Bounded* treatment is caused by the assumption of zero audit cost assumption in this setting. The intuition behind this is simple. When tax fraud generates extra revenues and audit resources are negligible, the auditor is always better-off spending all of the resources on catching cheaters. Therefore, this result implies that the bounded rule generates less net profit for the tax authority if the audit cost is sufficiently low.

Now turn to the *Bounded-Hq* treatment. If we use the cheating rate (i.e. the percentage of tax fraud in the entire population) instead of the underreporting rate for comparisons, we find no difference between the *Bounded-Hq* and the *Bounded* treatment, despite the fact that players have more opportunities to cheat in the *Bounded-Hq* treatment ($p = 0.342$ throughout 30 periods and $p = 0.248$ in the last 10 periods). Considering the number of audits is even less in the *Bounded-Hq* treatment, the result suggests that the cost-effectiveness of the bounded rule is still robust regardless of the base of comparisons.

Individual behavior

Figure 2.1 displays the distribution of the underreporting rate for H-type players across treatments. The horizontal axis represents subjects' underreporting frequency throughout the game (i.e. the percentage of times when they receive high income and decide to underreport). The vertical axis represents the proportion of players having similar underreporting frequency in each treatment.

Figure 2.1: Individual underreporting frequency distribution



The main message conveyed by Figure 2.1 is that theory has limited explanatory power over the individual-level data for the *Traditional* and *Bounded* treatments. Only 29.13% of the subjects in the *Traditional* treatment and 23.43% of subjects in the *Bounded* treatment behave exactly in accordance with theory. That is, they underreport whenever they receive high income throughout the experiment. The percentage of subjects who always report their income truthfully is 12.5% and 15.63%, respectively. [8] Even corrected for these players, the theory underpredicts the deterrence effect of both auditing rules. According to Figure 2.1, around 60 percent of the subjects switch between the two options with various levels of frequency. This pattern is the same in both treatments (Mann-Whitney test, $p = 0.322$). On the other hand, the distribution of the underreporting frequency *Bounded-Hq* is significantly different (Mann-Whitney test, $p < 0.05$). Only about 7% of all subjects choose to submit

[8] Since we do not elicit the beliefs of the players in the game, we do not identify whether a person who honestly states their income is intrinsically honest or conditionally honest.

"low-income" , while 33% of the subjects honestly state their income throughout the experiment.

Choice models under uncertainty

This section attempts to develop alternative models that explain the stochastic component of behavior. Theory based on individual profit maximization predicts that strategic players will always choose to submit "low-income" , while honest players will always report the type of income they receive. In either case, the choice should be consistent across periods. Figure 2.1 clearly indicates that is not the case. In the following, we exclude data from the *Bounded-Hq* treatment since learning adds too much dynamics and complicates the analysis.

We propose a model, the discrete-choice model, that allows us to better understand individual behavior in the experiment. The discrete-choice model is a framework to relax the perfect rationality assumption and to accommodate boundedly rational behavior (McFadden (2001)). Models in this framework are motivated by empirical studies in which observed decisions exhibit some noise (see, e.g., Fischbacher and Stefani (2007), Loomes (2005), Rieskamp (2008) and Wilcox (2010)). Such noise could come from observed sources like decision errors, but could also come from other unobserved or unmodeled channels such as individual perceptions of the game, or sensitivity to payoff changes. Due to the presence of such noise, people make decision errors and hence do not behave consistently with their choices. Our *Baseline* treatment is essentially a non-strategic choice-under-uncertainty problem for H-type players. Therefore, the classic individual discrete-choice model is a natural setting to explore behavioral anomalies. The bounded treatment introduces interactions of players. A general way to incorporate decision errors is the quantal response equilibrium first proposed by McKelvey and Palfrey (1995), which is based on the random utility-maximization model of McFadden (1973).

According to the discrete-choice framework, H-type players will choose to underreport if and only if the difference in the expected utilities is sufficiently large to exceed a stochastic error denoted by ε; i.e.,

$$EU(\pi_u) - \pi_h > \varepsilon.$$

In the expression, π_u and π_h denote the expected profits from underreporting and reporting honestly, respectively. The parameter ε is commonly assumed to be independently and identically distributed across players and actions with a Type 1 extreme value ("logit") distribution. The error can come from many sources, including the inability to calculate the expected payoff or trembling hands during decision making. A standard result of the discrete-choice model framework is that under the above error distributional assumptions, the underreporting probability $\widehat{b}$ is given by the relation below:

$$\begin{aligned}\widehat{b} &= \Pr EU(\pi_u) - \pi_h > \mu\varepsilon \\ &= 1/\{1+\exp[-(EU(\pi_u) - \pi_h)/\mu]\}. \end{aligned} \tag{2.1}$$

The parameter $\mu > 0$ captures the sensitivity of subjects' choices to the relative payoffs of the two choices. When μ approaches infinity, players choose underreporting and honest-reporting with equal probability, independent of the relative expected payoffs. When μ decreases, on the other hand, players put less probability weight on choices that yield suboptimal payoffs, and the probability that they make the optimal choice converges to 1 when μ approaches 0. Put differently, μ is an index of the measurement error when subjects calculate expected utility from underreporting.

Within this framework, this paper further relaxes the assumption of risk neutrality. In particular, three behavioral models are estimated and compared: risk-aversion, and loss aversion with and without combining probability weighting. In the risk-aversion model, subjects are assumed to have a CRRA-form utility function $u(\pi) = \left(\pi^{1-r}\right) / (1-r)$.[9] This model offers the possibility of explicitly testing the assumption of risk neutrality. If the estimated r is significantly different from zero, then the null hypothesis that subjects are risk neutral can be rejected.

While the observed compliance behavior can be explained by risk attitude, it is also consistent with the notion of loss aversion. Recent research has shown that that loss aversion provides a much better account of tax evasion both in the lab and in the field (see, e.g., Elffers and Hessing (1997), Yaniv (1999), King and Sheffrin (2002), Dhami and Al-Nowaihi (2007) and Dhami and Al-Nowaihi (2010)). The loss-aversion model characterizes individuals as loss averse in terms of reference income, denoted by R. For a given amount of money, $x > 0$, and the value function $v(x)$ (specified below), losses are weighted more than gains ($|-v(-x)| > v(x)$). This study follows Dhami and Al-Nowaihi (2007) and Dhami and Al-Nowaihi (2010) by taking the honest post-tax income as the reference point: $R = I_H - T_H$. The rationale for this reference point is as follows. If the reference point is selected differently, say, the initial income or the income after cheating detection, then taxpayers are always in the domain of losses or gains. In those cases, the asymmetry of gains and losses disappears, and the analysis falls back completely to an expected-utility framework.[10] The income relative to the reference point is as follows:

$$\pi_i = \begin{cases} I_H - T_H - F - R & \text{for} \quad i \text{ is caught.} \\ I_H - T_L - R & \text{for} \quad i \text{ is not caught.} \end{cases}$$

The form of the utility function follows Tversky and Kahneman (1992). It is defined

[9] Alternative utility forms such as CARA and power-expo utility do not change the fit of the data.

[10] More specifically, such a framework is called a rank dependent expected utility theory (RDEU), which can be considered as expected utility theory applied with a transformed cumulative probability distribution. See Dhami and Al-Nowaihi (2007) for more detail.

separately over gains and losses: $U(\pi_i) = \pi_i^{\alpha}$ if $\pi_i \geq 0$, and $U(\pi_i) = -\lambda(-\pi_i)^{\beta}$ if $\pi_i < 0$. The α and β are the parameters controlling for the curvature of the utility functions, and λ is the coefficient of loss aversion. Subjects are considered loss-averse if $\lambda > 1$.

Besides value functions, subjects could also have a nonlinear transformation of the probability scale (i.e. they overestimate low probabilities and underestimate high probabilities (see, e.g. Kahneman and Tversky (1979))). In order to examine the effect of subjective probability weight, this paper estimates a third model combining the loss-averse utility form with a probability-weighting function. In particular, this paper adopts a popular form of the one-parameter probability-weighting function: $w(p) = p^{\delta}/(p^{\delta} + (1-p)^{\delta})$, where $\delta \geq 0$. Note that if $\delta < 1$, the weighting function has an inverted " S" shape, which is concave for low probabilities and convex for high probabilities, and crosses the diagonal at the probability of 1/3.

Recall that H-type players are choosing between a safe lottery and a risky one with fixed probabilities in the traditional rule, but endogenous probabilities under the bounded rule. In the following, denote parameter "a" as the perceived audit probability in the *Bounded* treatment. The estimated parameter a answers the following question: If a bounded rule is transformed into the context of a traditional rule, which exogenous audit probability "a" best justifies behavior? Moreover, how do risk attitude, probability weighting, or loss aversion influence subjects' perception of the audit probability? The conditional log-likelihood is the following:

$$\begin{aligned}
\ln L &= \sum_{i,t} \left\{ y_{it} \ln \left(\frac{1}{1 + \exp[\pi_h - E(\pi_u)/\mu]} \right) + (1 - y_{it}) \ln \left(\frac{\exp[\pi_h - E(\pi_u)/\mu]}{1 + \exp[\pi_h - E(\pi_u)/\mu]} \right) \right\} \\
E(\pi_l) &= \begin{cases} 0.6 \times 22.5 + 0.4 \times 2.5 & \text{for} \quad i \in \textit{Traditional} \\ (1-a) \times 22.5 + a \times 2.5 & \text{for} \quad i \in \textit{Bounded} \end{cases}
\end{aligned}$$

where $y_{i,t} = 1(0)$ denotes that subject i underreports (reports honestly) in the tax-compliance game in period t. Table 2.3 reports the estimation results of various behavioral models.

To gain enough identification power, we pool data from both the risk elicitation task and the tax compliance game. Note that data of the tax compliance game contain two moments (i.e., the fraction of subjects selecting the "risky" lottery in the traditional rule and that in the bounded rule) given a fixed payoff structure. Hence, they do not have the identification power for more than two parameters. Incorporating risk elicitation data increases the identification power substantially since there are sufficient payoff variances among different lottery pairs.

Table 2.3: Comparison of behavioral models

	Pure noise		Risk aversion		Loss aversion		Loss aversion & Probability weighting	
	Traditional	*Bounded*	*Traditional*	*Bounded*	*Traditional*	*Bounded*	*Traditional*	*Bounded*
Risk magnitude r			0.366***	0.594***				
			(0.350)	(0.055)				
Gain domain curvature α					0.445***	0.428***	0.640***	0.533***
					(0.034)	(0.038)	(0.459)	(0.075)
Loss domain curvature β					0.548***	0.708***	0.586***	0.858***
					(0.052)	(0.030)	(0.068)	(0.073)
Loss-aversion coefficient λ					1.100***	1.148***	1.674***	1.283***
					(0.802)	(0.030)	(0.123)	(0.171)
Weighting parameter δ							1.150***	0.899***
							(0.193)	(0.120)
Perceived audit prob.a		0.490***		0.336***		0.305***		0.240***
		(0.006)		(0.017)		(0.007)		(0.023)
Noise μ	3.202***	1.079***	0.667***	0.618***	0.266***	0.256***	0.430***	0.289***
	(0.553)	(0.111)	(0.067)	(0.098)	(0.019)	(0.424)	(0.042)	(0.040)
Log-likelihood	-1458.662	-1299.671	-1163.773	-1087.292	-1141.710	-1082.473	-1141.353	-1082.111
Observations	2331	2287	2331	2287	2331	2287	2331	2287

Notes: *** 1% significance level, ** 5% significance level, * 10% significance level. To account for within group correlation, the standard errors are clustered on individuals.

At first glance, all parameters in these models are significant, suggesting that the alternative behavioral models help to explain the compliance behavior in our study. For instance, the risk-aversion specification suggests that subjects are risk averse in both treatments, as the CRRA coefficient r is significantly larger than zero. It indicates that risk aversion helps to explain our data. In fact, compared to a restricted model with pure noise, the risk aversion specification significantly increases the fit of the model for both treatments (Likelihood Ratio test: $p < 0.001$). The perceived audit probability for a risk-averse subject in the *Bounded* treatment is about 0.34. The explanation is straightforward. To induce a similar compliance pattern among subjects who are risk-averse, the audit capacity of the bounded rule can be set smaller, such that it induces the same deterrence effect compared to a traditional rule with audit probability $a = 0.336$. In other words, fewer resources are needed to achieve the same level of deterrence for risk-averse subjects as for risk-neutral ones.

Table 2.3 also shows that the loss-aversion specification increases the log-likelihood even more compared to the risk-aversion specification. Subjects in both treatments exhibit a mild degree of loss aversion. The coefficients of the loss-aversion parameter λ are larger than 1 in both treatments, which means that subjects are more sensitive to loss than to the equivalent magnitude of gain. The slopes of the value function indicate concavity in the gain domain (α) and convexity in the loss domain (β). Moreover, a Vuong test on non-nested models favors the loss-aversion model over the risk-aversion model ($p < 0.05$). If subjects are loss-averse, the bounded rule is even cheaper to implement, as the induced deterrence rate only needs to be the same as a traditional rule with audit probability $a = 0.306$.

The third specification combines loss-aversion utility and probability weighting. However, the likelihood of this specification does not improve significantly. Moreover, the probability-weighting parameter δ is not significantly different from 1 for both treatments ($p = 0.438$ and 0.397 respectively). This means that the average subjective probability of the subjects is pretty much in line with the objective audit probability. Overall, the results seem to indicate that the driving force for the observed compliance frequency is more likely to be found in the way they view losses and gains, rather than in how they assess probabilities.

Figure 2.2: Observed and predicted underreporting rates

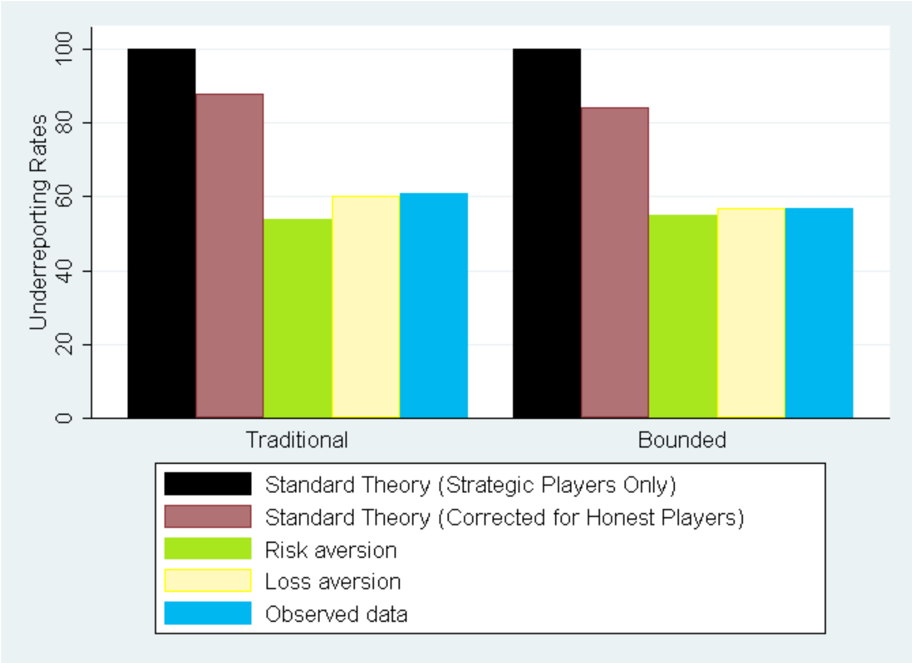


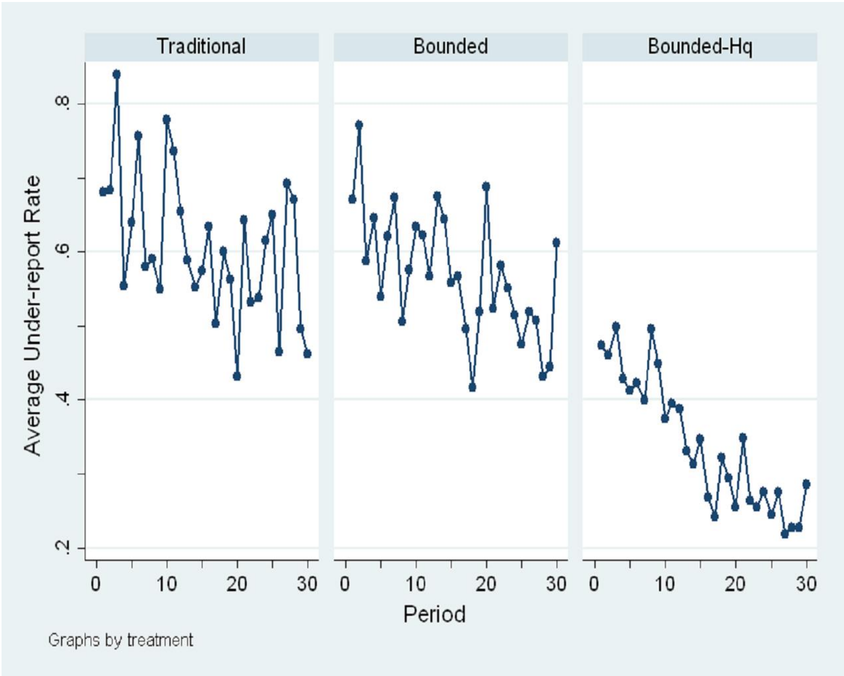
Figure 2.2 displays the observed and predicted underreporting rates based on risk- and loss-aversion models. Since estimation results suggest that probability weighting does not explain the data well, parameters are taken from the second specification of loss aversion without probability weighting. Among the three models, the one using loss aversion fits our data the best. Result 2.3 summarizes the section.

Result 2.3 *The proportion of compliance behavior in both treatments is consistent with the presence of loss aversion together with some stochastic decision errors, although not in probability weighting.*

2.6 Learning and social characteristics

Figure 2.3 depicts the average underreporting rates across treatments. The dynamics in the *Traditional* and *Bounded* treatments look similar. In contrast, the average underreporting rate in the *Bounded-Hq* is visibly lower and declines steadily over periods.

Figure 2.3: Underreport rate over 30 periods



In the post-experiment questionnaire, subjects are asked to provide social demographic information such as gender, nationality. We also elicit their risk attitude and tendency to be opportunistic (i.e. the Mach IV score). This information allows us to study how subjects form and adjust their underreporting decisions under different rules. The first specification concerns compliance behavior. We use the following random-effect probit model specification:

$$y_{it} = \gamma \times x_{it} + u_i + \epsilon_{it} \tag{2.2}$$

The variable y equals 1 if subjects decide to underreport, and is equal to 0 otherwise. Furthermore, x is a vector of explanatory variables, the u_i represent individual random effects and γ is a vector of parameters. The explanatory variables include subjects social backgrounds such as gender, nationality and experience of economics. They also contain a history of play such as underreporting performance in the previous period, period number and its square term.

Table 2.4: The influences of social background and learning on compliance behavior

	Dynamics and social characteristics			Social characteristics only		
	Traditional	*Bounded*	*Bounded-Hq*	*Traditional*	*Bounded*	*Bounded-Hq*
Underreport detection experience	-0.102	-0.498***	0.010	–	–	–
	(0.186)	(0.195)	(0.105)	–	–	–
Period	-0.039	-0.040	-0.082***	–	–	–
	(0.026)	(0.032)	(0.023)	–	–	–
Period^2	0.0006	0.0004	0.001	–	–	–
	(0.0008)	(0.0009)	(0.0006)	–	–	–
Degree of risk aversion	-0.102*	-0.325***	-0.237***	-0.220***	-0.295***	-0.210***
	(0.057)	(0.065)	(0.064)	(0.050)	(0.060)	(0.056)
Gender (1 for men)	0.756*	0.960*	0.459	0.733**	0.835*	-0.376
	(0.413)	(0.544)	(0.478)	(0.352)	(0.484)	(0.401)
Years of learning economics	0.052	0.797***	0.278	0.178	0.688***	0.261
	(0.186)	(0.287)	(0.214)	(0.159)	(0.262)	(0.185)
Econ experience × Game theory	0.083	-0.574***	-0.066	0.096	-0.518**	-0.088
	(0.144)	(0.274)	(0.187)	(0.119)	(0.249)	(0.160)
Dummy for Eastern Europeans	0.015	-0.409	0.125	-0.312	-0.611	0.477
	(0.899)	(0.922)	(0.925)	(0.764)	(0.826)	(0.825)
Dummy for Dutch	-0.102	1.190	0.620	0.012	1.106	0.802
	(0.691)	(0.777)	(0.677)	(0.596)	(0.702)	(0.737)
Dummy for Chinese	-0.234	1.336	-0.009	0.314	1.083	0.282
	(0.652)	(0.798)	(0.778)	(0.573)	(0.717)	(0.795)
Dummy for other Asian	-0.878	0.159	0.940	0.068	0.334	1.207
	(0.784)	(0.971)	(0.968)	(0.678)	(0.963)	(0.923)
Mach-IV score	0.025	-0.019	0.018	0.024*	-0.016	0.017
	(0.016)	(0.018)	(0.015)	(0.013)	(0.016)	(0.013)
Tax filing experience	-0.005	-0.304	-0.339	(-0.154)	-0.426	-0.176
	(0.458)	(0.492)	(0.575)	(0.389)	(0.436)	(0.462)
Constant	-0.508	5.318**	0.918	-0.028	4.509***	-0.510
	(2.091)	(2.308)	(1.800)	(1.599)	(1.973)	(1.702)
Log-likelihood	-444.868	-359.410	-640.430	-455.741	-386.041	-720.591
Observations	957	912	1670	987	943	1725

Notes: *** 1% significance level, ** 5% significance level, * 10% significance level. To account for within group correlation, the standard errors are clustered on individuals.

The panel on the left presents the impact of both social characteristics and learning experience in the game. The panel on the right only considers the impact of social characteristic information alone. Various effects are found in the regressions. The first one concerns the effect of learning. In the *Bounded* treatment, detection experience in the previous round decreases non-compliance propensity. Interestingly, players with a background in economics are more likely to underreport, which seems to suggest that training in economics results in behavior more in line with homo-economicus. These effects, however, do not exist in the other two treatments.

The only social characteristic information which has a consistent impact on underreporting decision is risk attitude, i.e. the total number of safe lotteries selected in the risk elicitation task. In all treatments, with and without controlling for learning, the number of safe lotteries selected is negatively correlated with the propensity of underreporting. In other words, the more risk averse a subject, the less likely it is that he or she cheats in the tax evasion game.

In the *Traditional* and *Bounded* treatments, men are more likely to underreport than women are, regardless whether we control for learning or not. This is consistent with a general pattern that men are less risk averse than women. Interestingly, this impact of gender is no longer significant in the *Bounded-Hq* treatment. Apart from these variables, no other social demographic information affects behavior significantly and consistently.

2.7 Discussion and conclusion

This paper examines the bounded rule as a more realistic representation of the auditing procedure which naturally integrates social interactions and interdependent beliefs among taxpayers. Constrained by a budget, it audits all "red-flagged" reports whenever the total number of these reports is no more than the maximum number of audits allowed by the budget, and merely the maximum otherwise. In the parameter regions where it induces "all-or-none" outcome in compliance, it is equivalent to a traditional rule. In addition, there exists a region where the bounded rule induces coordination among taxpayers.

The paper then examines these properties of the bounded rule in a controlled laboratory experiment. Subjects receive either high or low income with a predetermined probability. On knowing a certain auditing rule (traditional or bounded), they report income to the tax agency. We construct three treatments: a traditional rule, a bounded rule with an unique Nash equilibrium and a bounded rule which induces coordination. The experimental results indicate that the compliance rate in the bounded rule is the same as that in the traditional rule. The deterrence effect of a bounded rule becomes much stronger where it introduces multiple equilibria. It deters subjects from coordinating on the payoff-dominant equilibrium with a low implementation cost. The above result indicates that strategic uncertainty aversion is an independent source of deterrence in this setup.

In the first two treatments, participants in our experiments do not follow the sharp predictions of the model. The compliance rates are higher than the prediction, even taking into account the 10% to 15% players who consistently report their income honestly. About 60% of the subjects switch their decisions alternatively. To better account for this, we adopt a bounded rational model incorporating noise into decision making. In this framework, we estimate and compare several choice models. Among various specifications, loss aversion combined with stochastic errors are more successful at tracking observed data patterns.

The results from the auditor revenue comparisons also suggest that the net revenue collected under the bounded rule is lower than under the traditional rule if cost per audit is sufficiently low. Catching tax cheaters brings in extra revenue for tax agencies. As a result, it is worth investing the effort when the cost of doing so is low. That said, tax auditing in reality is indeed costly, not only because of the expenditure in hiring and training auditors, but also the complicated and time-consuming auditing procedures. Furthermore, a recent paper by Feld et al. (2010) suggest that a more aggressive auditing program may lead to a crowding out effect of tax morale and thus an increased shadow economy in Germany. A less aggressive auditing strategy supported by the bounded rule, on the other hand, could help to reduce the distrust of taxpayers toward tax agencies.

This study is just a first step into the investigation of the bounded rule. In our current setup, taxpayers can only decide whether to underreport or honestly report. In future studies, the model could be extended to allow choices on the extent of underreporting. Another possible extension might involve introducing a human auditor to further examine the strategic interactions. This would be useful, due to the fact that taxpayers can communicate with each other in reality. Alm and McKee (2004) show that such cheap-talk communication could help taxpayers to coordinate on zero-compliance (payoff-dominant) equilibrium. However, if a strategic auditor could observe this, s/he would be able to adjust the audit capacity accordingly to combat collusion among taxpayers.

2.A Appendix

Proofs

Proof of Proposition 2.1

Let $\bar{K} = [\bar{a}N]$, as $\bar{K}$ needs to be an integer. Thus, $a_{BD} = \min\{1, \overline{K}/L\} = \min\{1, [\bar{a}N]/L\}$. Since $L \leqslant N$, $a_{BD} \geqslant \bar{a}$. That means, in the scenario where all players declare low income, the audit probability a_{BD} is equal to $\bar{a}$. The H-type players are indifferent between the decisions of underreporting and reporting honestly. If $K > \bar{K}$, that means the lowest probability of being audited is strictly larger than $\bar{a}$. Hence, any $K > \bar{K}$ is sufficient to support full compliance.

The simplest case to induce zero compliance is to set $K = 0$. Because of zero audit, self-regarding, profit-maximizing H-type players always report low income, regardless of their beliefs towards other H-types. More generally, if $K < [\bar{a}]$, the bounded rule cannot induce any compliance for strategic players regardless of the income distribution. In other words, in the worst-case scenario in which only one H-type player claims low income, the audit probability he or she faces is lower than $[\bar{a}]$. Hence, strategic H-type players will underreport.

Proof of Proposition 2.2

This subsection contains two parts. The first part proves that given that all players are rational, strategic expected profit maximizers, the game introduced by the bounded rule is dominance solvable. The second part shows that this claim still holds by introducing conditionally or intrinsically honest players.

The proof is trivial that reporting high income is a dominated strategy for the L-type players. To prove that the best response of H-type players is underreporting given that L-type players display dominance, the expected payoff from underreporting should be strictly larger than the sure payoff from reporting truthfully. Moreover, this holds regardless of the beliefs that H-type players hold towards the other H-types.

First assume that an H-type player anticipates that nobody else will underreport. That is, $\overline{b_0} = (b_1, b_2, ..., b_{N-1}) = (0, 0, \ldots, 0)$. In this situation, "low-income" reports are submitted by L-types. Since the probability of being an L-type is $q = 0.5$ for every other player, the probability that exactly n out of $N-1$ players submit "low-income" reports follows the binomial distribution $\mathbf{Bin}\ (n, N-1; q) = \mathbf{Bin}\ (n, 7; 0.5)$. Let π_F denote the profit of a tax cheater if caught (€2.5), and π_S the profit of a cheater if not caught. The expected payoff from underreporting π_l is:

$$E(\pi_l|\overline{b_0}) \quad = \quad \sum_{n=0}^{N-1} \mathbf{Bin}(n; N-1, q) \times \{\min(\frac{2}{n+1}, 1) \times \pi_F + [1 - \min(\frac{2}{n+1}, 1)] \times \pi_S\}$$

$$
\begin{aligned}
&= \pi_S - (\pi_S - \pi_F) \times \sum_{n=0}^{N-1} \mathbf{Bin}(n; N-1, B_i) \times \min(\frac{2}{n+1}, 1) \\
&= 22.5 - 20 \times \sum_{n=0}^{7} \mathbf{Bin}(n; 7, 0.5) \times \min(\frac{2}{n+1}, 1) \\
&= 12.698
\end{aligned}
$$

The sure payoff of reporting truthfully is 12.5. Hence, a self-interested, risk neutral H-type player will underreport.

The remaining proof shows that for any given set of beliefs held by an H-type player, the expected payoff from underreporting is always not less than $E(\pi_l|\overline{b_0})$. Assume that player N thinks the first $N-1$ players underreport with probability $\overline{b} = (b_1, b_2, ..., b_{N-1})$. The probability that player i submit "low-income" is $B_i = 1 - q + qb_i = \frac{1}{2}(1 + b_i)$. Note that $B_i \in [\frac{1}{2}, 1]$. To facilitate notation, define an index vector $\mathbf{I} = (i_1, i_2, ...i_7)$, with $i_1 \neq i_2 \neq ...i_7$. Each index takes a value from the set $\{1, 2, ..., 7\}$. The probability that n out of 7 other players submit " low-income" reports is:

$$Pr(n|\overline{b}) = \sum_{s=1}^{C_7^s} \sum_{j=1}^{s} B_{i_j} \sum_{k=s+1}^{i_7} (1 - B_{i_k})$$

The expected payoff from underreporting is therefore:

$$E(\pi_l|\overline{b}) = \sum_{n=0}^{N-1} Pr(n|\overline{b}) \times \{\min(\frac{2}{n+1}, 1) \times \pi_F + [1 - \min(\frac{2}{n+1}, 1)] \times \pi_S\}$$

It turns out that for any given b_i, $\partial E(\pi_l)/\partial b_i = (\partial E(\pi_l)/\partial B_i) \cdot (\partial B_i/\partial b_i) > 0$.[11] This means that the expected payoff from underreporting is increasing in the (subjective) propensity to evade taxes. Hence, given any set of beliefs $\overline{b} = (b_1, b_2, ..., b_{N-1})$, $E(\pi_l|b) \geq E(\pi_l|\overline{b_0})$. Hence, the best response of the H-type players is to underreport.

The second part of this subsection proves that the introduction of conditionally or intrinsically honest players does not change the directions of treatment difference.

Let ρ be the probability that a player is conditionally honest, and $1 - \rho$ be the probability that a player is a strategic, self-regarding profit maximizer, where $0 \leq \rho < 1$. We do not allow $\rho = 1$, since at least one strategic player is thinking of this problem. In our setting, in particular, the number of conditionally honest players ρN can be any number from 0 to 7 out of 8 players. We further assume that the ρ is the same in both treatments.

The strategy of the conditionally honest players is as follows. When they receive low income, they will always report truthfully. When they receive high income, they will honestly report their income if they think the number of other players cheating

[11] Calculation is available upon request.

on taxes $(1-\rho)N$ is not higher than a certain threhold $\lambda \in [0,7]$, and underreport their income otherwise.

To prove the statement, we only need to show that the inclusion of honest players does not affect the strategy of the profit maximizers. When the strategic players are assigned to be L-types, they gain a higher payoff by reporting truthfully, regardless of the auditing rule implemented. In the *Traditional* treatment, H-type profit maximizers only compare a certain payoff of reporting truthfully and the expected payoff from the tax evasion gamble if they underreport. Hence, the existence of honest players will not affect their choices. In the *Bounded* treatment, the subjective beliefs of strategic, H-type players of the number of "low-income" reports now become: $B_i = (1-q) + q(1-\rho)b$. Given that $q = 0.5$, $0 \leq \rho < 1$, B still lies in the interval $[\frac{1}{2}, 1]$. Therefore, Proposition 2.2 still holds.

Anticipating that strategic profit maximizers will cheat when they receive high income, the conditional honest players will assess the self-interested profit maximizers in the population. If the proportions $(1-\rho)N \leq \lambda$, they will honestly report their income. If $(1-\rho)N > \lambda$, they will underreport.

We assume that belief is mutually rational in equilibrium. Hence, in the presence of conditionally honest players, the non-compliance rates of both treatments become:

$$\sum \mathbf{Bin}(n;N,q)(1-\rho) = \begin{cases} (1-\rho) & \text{if} \quad (1-\rho)N \leqslant \lambda \\ 1 & \text{if} \quad (1-\rho)N > \lambda \end{cases}$$

The analysis of intrinsically honest players are simpler since their strategies could be reformulated by setting $\lambda = 7$. As $(1-\rho)N \leqslant 7$ always holds, the compliance rates of both treatments with intrinsically honest players become:

$$\sum \mathbf{Bin}(n;N,q)(1-\rho) = (1-\rho)\ .$$

Proof of Proposition 2.3

Let $\sigma_i(j)$ be the probability that type i player (H-type or L-type) will use strategy j (u or h). There are two pure Nash equilibria and one mixed-strategy equilibrium in this treatment:

$$\{(\sigma_H(u) = 1, \sigma_L(h) = 1), (\sigma_H(h) = 1, \sigma_L(h) = 1), (\sigma_H(u) = 0.432, \sigma_L(h) = 1)\}.$$

In other words, the two pure Nash equilibria are 1) all H-type players underreport and 2) all H-type players honestly report. L-type players always honestly report.

Let us examine the former case. Given that an H-type player thinks that all other H-types choose strategy u, s/he will have an expected payoff of 17.5 by playing strategy l. By deviating to h, the payoff decreases to 12.5. Since we assume symmetry among players, no one has an incentive to deviate from underreporting, which constitutes an NE.

A highly similar analysis applies to the latter case. Given that all other H-type players play strategy h, a strategy deviation from h to l will yield a lower expected payoff for H-type players (from 12.5 to 3.59). Hence, no one has an incentive to deviate.

On top of the two pure equilibria, the game also has a symmetric mixed-strategy equilibrium in which each H-type player is indifferent between the strategy of honest-reporting and underreporting. Given the game parameters, the underreporting probability b that induces utility indifference is $b^*_{SE} = 0.432$.

Experiment instructions

Instructions comparison

The instructions given in the next subsection are for the *Bounded* treatment. These instructions differ from those given for the other treatments as follows:

- *Traditional* treatment

  1. The second bullet (concerning matching protocol) of the list under "Task Description" in the instructions for the "Tax Compliance Game" is absent.
  2. The "Audit Probability Table" is absent.
  3. The phrase "see audit prob. table" in the "Payoff Table" becomes 0.4.

- *Bounded-Hq* treatment

  1. In the third bullet of the list under "Task Description" in the instructions for the "Tax Compliance Game" , the probability of receiving €25 becomes 0.9, and accordingly the probability of receiving 10 becomes 0.1.
  2. In the "Payoff Table" (immediately before "Payment Method" in the instructions for the "Tax Compliance Game"), the probabilities in the second column become 0.9 and 0.1, respectively.

2.A.1 Instructions of the *Bounded* treatment

- Please read these instructions carefully!
- Please do not talk to your neighbours and remain quiet during the entire experiment.
- If you have a question, please raise your hand. We will come to you to answer it.

- You will receive a show-up fee of €3 for completing all tasks in the experiment, independent of your performance.

Task Description

- This session consists of 30 periods of play; each period is completely independent of the others.
- Of the participants in the room, two groups of 8 participants will be randomly formed at the beginning of each period. You will not know the identity of the other players in your group in any period.
- At the beginning of each period, you will receive a taxable income of either €25 or €10. The probability of receiving €25 is 0.5; the probability of receiving €10 is 0.5.
- Your task is to report your income to the auditor, which is played by a computer. The amount that you report is your decision. You can report either €25 or €10, regardless of your received income.

After-tax Income Determination

Your after-tax income in this period is determined by the following two steps: tax payment and an audit.

Step One: Tax payment

The tax rate is 50% for those who reported €25 and 25% for those who reported €10. Suppose the income you received is €25:

- If you report €25 to the auditor, the auditor will charge €12.5 (50% of €25) as tax. So your after-tax income in this period equals €25 – €12.5 = €12.5.
- If you report €10 to the auditor, the auditor will charge €2.5 (25% of €10) as tax. So your after-tax income in this period equals €25 – €2.5 = €22.5.

Suppose the income you received is €10:

- If you report €10 to the auditor, the auditor will charge €2.5 (25% of €10) as tax. So your after-tax income in this period equals €10 – €2.5 = €7.5.
- If you report €25 to the auditor, the auditor will charge €12.5 (50% of €25) as tax. So your after-tax income in this period equals €10 – €12.5 = -€2.5.
- In sum, the auditor charges tax based on your reported income, instead of your received income.

Step Two: Audit

The auditor does not know your received income unless your report is audited later.

Auditing procedure:

- If your reported income is €25, it will not be audited. That means what you have earned in step one (€12.5 or -€2.5) will be your after-tax income (if your received income is €25 and €10, respectively).
- Regardless of your received income, if your reported income is €10, there is a chance that your report will be audited. The outcome is as follows:
  - Suppose your reported income is €10 AND your received income is also €10. Then what you have earned in step one (€7.5) will be your after-tax income, no matter whether your report is audited or not.
  - Suppose your reported income is €10 AND your received income is €25. If your report is not audited, you will keep the €22.5 earned in step one; if audited, you will get €2.5.

Auditing probability:

- The number of reports the auditor will audit depends on the number of players reporting an income of €10 in a group.

  - If the number of €10 income reports is equal to two or less, the auditor will audit all of the €10 reports.

  - If the number of €10 income reports is three or more, then two out of such reports will be randomly selected for audit.

  - The "Audit probability table" below shows the audit probabilities for a player who reported an income of €10.

Audit Probability Table

Number of €10 reports	0	1	2	3	4	5	6	7	8
Audit Probability	100%	100%	100%	66.7%	50%	40%	33.3%	28.6%	25%

- The "Payoff Table" below summarizes all of the possible scenarios you may encounter in one period and the related payoffs:

Payoff Table

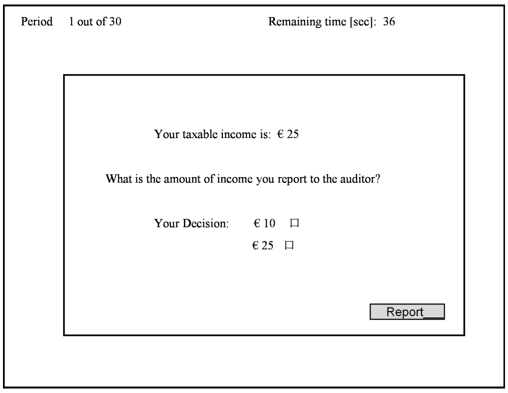
Received Income	**Probability**	**Reported Income**	**Audit Probability**	**After-tax Income if audited**	**After-tax Income if NOT audited**
€25	0.5	€25	0	€12.5	€12.5
		€10	see audit prob. table	€2.5	€22.5
€10	0.5	€10	see audit prob. table	€7.5	€7.5
		€25	0	−€2.5	−€2.5

Payment Method

- At the end of this experiment, one out of 30 periods will be selected to determine your payoff for this task. The computer program will generate a random number from 1 to 30. This number will determine one of the 30 periods. Your performance in that period determines your payoff.
- You will be paid based on your after-tax income for the randomly selected period.
- Because each period is equally likely to be selected for payment determination, you should make your decision in each period as if that period would be selected for payment.
- Your payoff will be paid out in cash at the end of the experiment along with your earnings in the other task(s).

We will now show you what the computer screens look like.

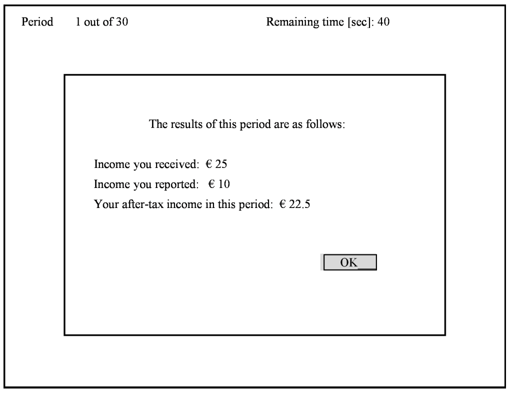
SCREEN 1



In "Screen 1" , you can decide the amount of income to report to the auditor. Please select either "€10" or "€25", and confirm your choice by pressing the "Report" button.

Warning: Before pressing the button, make sure your choice is correct. You cannot change your decision after you have pressed OK.

SCREEN 2



" Screen 2" is the feedback table you will receive regarding your after-tax income. You will find information on the initial taxable income you received, the income you reported and your after-tax income in this period.

Click on OK when you finish checking the information.

Note that the purpose of the screen shots is to clarify the procedure, rather than to provide advice about how to act. You should make the decisions that are best for you.

Risk Elicitation Task[12]

Task Description

In this task, you are asked to make decisions related to 21 choice pairs. In each choice pair, you need to select between two lotteries labeled "Lottery A" and "Lottery B". Please, take your time and read each choice pair carefully. An example of a typical choice pair is given below:

Choice No.1	Lottery A	€5.5 with probability 0.5 or €3.5 with probability 0.5	Your choice:	Lottery A	☐
	Lottery B	€9 with probability 0.5 or €0.5 with probability 0.5		Lottery B	☐

Payment Method

- You need to make choices for all 21 choice pairs. However, only one of the 21 choices you have made will be chosen for the payoff determination of this task. First, the computer program will generate a random number from 1 to 21. This number will determine a choice pair. Then, the computer program will simulate the lottery you have chosen and reveal the outcome on your screen. The outcome of this lottery will determine your payoff.
- For example, suppose that the computer program has generated a random number 2. It will then check what you have selected in choice pair number 2. Suppose that you have chosen Lottery A in that choice pair. Then the computer program will simulate Lottery A and reveal your payoff (either €5.5 or €3.5). Your payoff will be paid out in cash at the end of the experiment along with your earnings for the other task.

It is important that you fully understand the lottery selection task. Please raise your hand if you have any questions at this moment.

[12]The risk elicitation task is conducted after the tax-compliance game. However, the subjects do not know the existence of this task when they are playing the tax-compliance game.

Post-experimental Questions

Questions on Treatment Manipulation

Please evaluate the following statements with respect to the tax reporting task:[13]

1=strongly disagree, 2=somewhat disagree, 3=slightly disagree, 4=no opinion, 5=slightly agree, 6=somewhat agree, 7=strongly agree

1. The instructions were clearly formulated.
2. I felt that I performed well on the task.
3. I received plenty of time to carry out the task.
4. I was motivated to do well on the task.
5. The task was fun to perform, motivating me to achieve a payoff as high as possible.
6. I considered the tax reporting task to be fairly complex.
7. My payoff is determined not only by my own decision, but also by the decisions of the other players.
8. When making my decision, I thought about what other players might do.
9. I feel obliged to report the received income in each period.
10. The chance I have received €25 is about 50%.[14]

Questions on Background Information

Please answer the following survey questions. Your answers will be used for this study only. Individual data will not be exposed.

1. What is your gender?
2. What is your nationality?
3. How many years have you already studied in economics?
4. Have you ever had a course related to game theory?
5. Have you ever had a part-time job?

[13]The first five questions are used to understand the subjects' perception about the experimental setup and instructions in general. We do not expect to find differences across treatments. The last five questions focus on capturing different types of manipulations of the treatments; therefore, we expect to see differences across manipulations.

[14]In the *Bounded-Hq* treatment, the chance should be 90%, instead of 50%.

Questions on Mach IV Scale[15] In the following you will find a list of statements. Please read them carefully and indicate to what extent you agree or disagree. Even if in some cases you would like to say that your answers depend on the circumstances, you should only choose one of the answers. Since all responses are anonymous you can answer freely. There is nobody on whom you need to make a good impression. Only if you answer very honestly can the results be used.

> *1=strongly disagree, 2=somewhat disagree, 3=slightly disagree, 4=no opinion, 5=slightly agree, 6=somewhat agree, 7=strongly disagree*

1. Never tell anyone the real reason you did something unless it is useful to do so.
2. The best way to handle people is to tell them what they want to hear.
3. One should take action only when sure it is morally right.
4. Most people are basically good and kind.
5. It is safest to assume that all people have a vicious streak and it will come out when they are given a chance.
6. Honesty is the best policy in all cases.
7. There is no excuse for lying to someone else.
8. Generally speaking, people won't work hard unless they're forced to do so.
9. All in all, it is better to be humble and honest than to be important and dishonest.
10. When you ask someone to do something for you, it is best to give the real reasons for wanting it rather than giving reasons which carry more weight.
11. Most people who get ahead in the world lead clean, moral lives.
12. Anyone who completely trusts anyone else is asking for trouble.
13. The biggest difference between most criminals and other people is that the criminals are stupid enough to get caught.
14. Most people are brave.
15. It is wise to flatter important people.
16. It is possible to be good in all respects.
17. Barnum was wrong when he said that there's a sucker born every minute.

[15] Questions 3, 4, 6, 7, 9, 10, 11, 14, 16 and 17 are reverse coded.

18. It is hard to get ahead without cutting corners here and there.
19. People suffering from incurable diseases should have the choice of being put painlessly to death.
20. Most people forget more easily the death of their parents than the loss of their property.

CHAPTER 3

Peer punishment in a social dilemma game with third-party approval [1]

3.1 Introduction

Punishment is a common means of enforcing cooperation in a society. A large number of laboratory studies have been conducted to investigate the effect of peer punishment mechanisms where the punishment proposers are also the enforcers (see, e.g., Fehr and Gächter (2000), Fehr and Gächter (2002), Fehr and Fischbacher (2004a), Masclet et al. (2003), Falk et al. (2005), Nikiforakis (2008)). In this paper, we take a first step to understand how sanctions promote cooperation when punishment enforcers are independent of proposers.

In many situations in a society, the party proposing sanctions toward wrong-doers is often not the one implementing sanctions. Litigation is a typical example. When a party (say, a cyclist) files a suit against another (say, a careless car driver who injures the cyclist), judges have the right to uphold the damage compensations. Arbitrators or mediators widely exist as cheaper and more flexible alternatives to the courts. Upon hearing a case like contract negotiation or product liability, arbitrators impose a legally binding decision on the disputants. Even in societies where codes of conducts are not formally written down as laws, tribal leaders often act as judges to settle disputes among other tribe members. Informal third parties are also commonly seen in

[1] This chapter is joint work with Erte Xiao. We thank Daniel Houser, Martin Kocher, Wieland Mueller, Charles Noussair, Jan Potters, Arno Riedl, as well as participants at the Carnegie Mellon University, Tiber work-in-progress seminar, M-BEES 2011 conference, the 10th Tiber Symposium on Psychology and Economics, and the 2011 ESA European conference in Luxembourg for valuable comments. We gratefully acknowledge the financial support from CentER lab funding at Tilburg University. Maolong Xu and Bastian Henze provided excellent assistance for the experimental sessions.

modern organizations. Project managers or team leaders, for instances, are responsible for reconciling disagreement and accusations among team members by coming up with a joint decision that applies to everybody.

Previous studies on third-party punishment assume third parties can directly punish other players after observing the outcome of a game (see, e.g. Fehr and Fischbacher (2004b), Kurzban et al. (2007), Charness et al. (2008), Xiao and Kunreuther (2010)). Emotions such as anger (to norm violators) and guilt (the responsibility to punish norm violators) can trigger third-party punishment (Nelissen and Zeelenberg (2009). However, we are not aware of a setting where punishment proposals could only be implemented upon approval from an independent third party.

In this paper, we argue that two possible factors could influence the performance of a third-party approval mechanism.

On the one hand, requiring an independent third party's approval can improve the efficiency of a peer punishment mechanism. More specifically, the presence of a third-party judge helps to reduce antisocial punishment. Experimental studies have documented evidence of antisocial or perverse punishment (that is, punishing the cooperators) when peer punishment is allowed (e.g. Casari and Luini (2009), Cinyabuguma et al. (2006), Rand et al. (2008), Herrmann et al. (2008), Nikiforakis (2008), Ertan et al. (2009)). Motives such as vengeance or spite trigger antisocial punishment (Herrmann et al. (2008), Nikiforakis (2008), Ertan et al. (2009), Xiao (2010)). Hence, these studies argue for the importance of "shunning retaliation and centralizing punishment in the hands of the state" (Herrmann et al. (2008)). A third party, as an outsider, is more likely to veto antisocial punishments as his judgment is less likely to be influenced by vengeance and more likely to be consistent with the existing social norms (Cubitt et al. (2011), Fehr and Fischbacher (2004a), Xiao and Kunreuther (2010)). As a result, punishment proposals targeting cooperators can be rejected by norm-seeking third parties. Hypothesis 3.1 sums up the discussion above:

Hypothesis 3.1 *A third-party approval mechanism increases the efficiency of a peer punishment mechanism by decreasing antisocial punishment.*

On the other hand, although less emotional third parties can make better judgments based on social norms, they can also reduce the punishment's severity level, leading punishment to be less effective. Previous studies support the view that costly punishment is used to express negative emotions (e.g. Fehr and Gächter (2002), Sanfey et al. (2003), Xiao and Houser (2005), Hopfensitz and Reuben (2009)). However, the transmission of emotion via punishment might be less effective if it needs approval from a third party. An emotionally-detached third party is likely to turn down severe punishment proposals for efficiency reasons, even though these proposals target defectors. In response to this, players of the game might adjust their punishment proposals. That is, they might decrease the severity of the punishment to increase the success rate, and therefore reduce the effectiveness of punishment. In summary, Hypothesis 3.2 states:

Hypothesis 3.2 *A third-party approval mechanism decreases the efficiency of a peer punishment mechanism by decreasing punishment of defectors.*

Note that the above two hypotheses point in different directions with respect to the effectiveness of a third-party approval mechanism. As a first step to study which effect dominates, we compare the effectiveness of a third-party approval mechanism to the one from second-party punishment in a two-person prisoner's dilemma experiment. In the baseline, players can directly impose costly sanctions after observing each other's actions. In the target treatment, players make punishment proposals to an independent third party. Punishment will be implemented at the proposer's cost if the third party approves it.[2] This third player does not participate in the social dilemma game, nor is his earnings associated with the outcomes or the judgment decisions. [3]

We find that both cooperation rates and earnings are significantly lower in the third-party approval treatment compared to the baseline. Although the third parties reject both antisocial punishment, they also reduce the punishment for defectors. The empowerment of a third party also decreases the proposed punishment for defectors. Since antisocial punishment only consists of a small proportion of all data, the negative effect overtakes the positive one, deteriorating cooperation in our setting. The results suggest that the intervention of a third party is a double-edged sword. It can reduce the effectiveness of the peer punishment mechanism in promoting cooperation (as we find in our setting) but it might help to promote cooperation if antisocial punishment is pervasive.

[2]In naturally occurring environments, institutions can empower the third party in various ways. For example, the third party can have the right to revise the punishment. As a first step, we adopt a binary decision mechanism to understand the impact of the third party's intervention. We find the cooperation rate is lower with the third party's intervention as the punishment is less severe. It is possible that the cooperation rate could be higher if the third party could, say, double the proposed punishment amount. In our experiment, we do not observe sufficient evidence showing the third party's decisions vary with on the proposed punishment amount. For example, punishment proposals on defectors lower than 6 tokens are approved in 57 out of 107 cases (53.3%) and those higher than 6 tokens are approved in 118 out of 184 cases (64.1%). The correlation between approval rate and proposed punishment amount is not significant. This implies that even allowing the third party to revise the proposal may not promote cooperation in our setting. Further studies are needed to understand how different ways of empowering the third party influence the outcome.

[3]Previous studies on the beahvior of an impartial third party suggest that they behave in accordance with the existing social norm even without any monetary incentives. On the other hand, incentivising the decisions of the third parties turns them into stakeholders and hence opens the possibility of a bias in their judgment. For instance, Xiao (2010) finds that third parties in a sender-receiver game punish lying senders truthfully in the absence of monetary reward from the decision. However, when they can benefit monetarily by punishing a sender, most third parties increase punishment to the sender regardless of the message sent. As a result, deception occurs more often compared to the baseline where third parties are not stakeholders of the game. More generally, the use of non-incentivized methods, such as survey questions, is standard in the study of social attitudes (see, e.g. Cubitt et al. (2011).)

The remainder of the chapter is organized as follows. In Section 3.2, we describe the experiment design and procedure. In Section 3.3, we present an analysis of the data. Finally, in Section 3.4, we make some concluding remarks.

3.2 Experiment

3.2.1 Design

As shown in Table 3.1, our experiment consists of two treatments: third-party observer (baseline) and third-party approval (TPA). The only difference between these two treatments is that in the baseline, the third party observes the experiment. In the TPA treatment, the third party plays the role of a judge in that he will decide whether to implement the punishment decision proposed by the players.

Table 3.1: Treatment design

Third-Party Observer (Baseline)	**Third-Party Approval (TPA)**
37 groups	38 groups
(2 players and 1 observer)	(2 players and 1 third-party judge)

In both treatments, subjects are randomly assigned to a group of three and remain in the same group and the same role throughout the experiment. To facilitate discussion, we name them person A, person B and person C. Each treatment consists of 20 periods and each period consists of two stages. In the first stage, person A and person B decide simultaneously whether to cooperate in a standard prisoner's dilemma game (see Table 3.2).

Table 3.2: Payoff table of the prisoner's dilemma game

		Person B	
		Option I	Option II
Person A	Option I	30, 30	15, 40
	Option II	40,15	20,20

If both players choose to cooperate, they both earn 30 tokens. If both of them defect, then they both earn 20 tokens. If one chooses to defect while the other cooperates, the defector earns 40 tokens while the cooperator earns 15 tokens. The exchange rate is 40 tokens to 1 euro.

In the second stage, after seeing each other's choices and earnings in the first stage, A and B simultaneously and independently decide how many tokens they want to deduct from each other's accounts. Every three tokens deducted from the other's account costs a player one token. In the baseline treatment, the punishment decisions are implemented immediately. Person C is simply an observer of the experiment. In the third-party approval treatment (TPA), C has to decide whether to implement the punishment decisions proposed by A and B, if any. A and B's punishment decisions will be implemented only if C has approved them. In both treatments, all the decisions are revealed to each player at the end of each period. In particular, to keep the information symmetry between the two treatments, A and B can still see each other's punishment decisions even when C vetoes the proposals. C's earnings in both treatments are independent of the outcome of the game. The program randomly draws a number from the prisoner's dilemma payoff matrix (15, 20, 30, 40) with equal probability. Moreover, person C only knows his own earnings at the end of the experiment, while person A and B never know the actual earnings of person C. All the above is common knowledge to every player in the experiment. This setting is to minimize the possibility of C comparing his own earnings with those of the two other players' during judgment.

3.2.2 Procedure

We conducted the experiment at the CentER lab at Tilburg University in 2011. A total of 225 students participated as subjects in the experiment. Each subject only participated in one treatment. Subjects were paid the total amount they earned over the 20 periods and the average earnings were €12.90. The experiments were programmed and conducted in Z-tree (Fischbacher (2007)). At the beginning of each session, a group of 15 to 18 subjects were randomly assigned to the computer terminals. The experimenter read the instructions aloud to ensure everyone got the same information. Before the experiment started, subjects had to answer all quiz questions correctly to make sure that they understood the rules of the game.

3.3 Results

3.3.1 Aggregate cooperation and earnings

Figures 3.1 and 3.2 plot the dynamics of the average cooperation rates and earnings over 20 periods of the play for person A and person B in both treatments. Cooperation rates are constantly lower in the TPA treatment compared to the baseline treatment (0.47 vs. 0.70, Mann-Whitney test, $p < 0.05$ [4]). So are average earnings except for the first and last period of the game (23.52 vs. 25.30, $p < 0.10$). Figure 3.1 also suggests subjects anticipate the treatment effect. The cooperation rate of period 1 in the TPA treatment is lower than the baseline (0.49 v.s. 0.65, $p < 0.05$). To understand the ob-

[4] All the non-parametric tests reported in this paper are two-sided Mann-Whitney rank-sum tests. In the analysis, we treat each group as an independent observation.

served treatment effect on cooperation, we first show that our data is consistent with the previous findings of the punishment effect on cooperation and then compare the punishment decisions between the two treatments. We then study how third parites approve punishment, as well as players' reactions after being punished.

Figure 3.1: Average cooperation rate over period by treatment

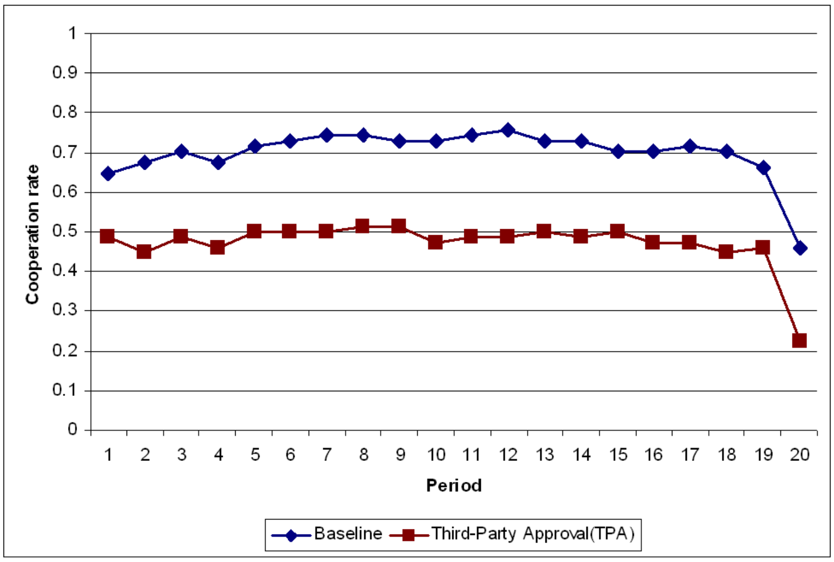
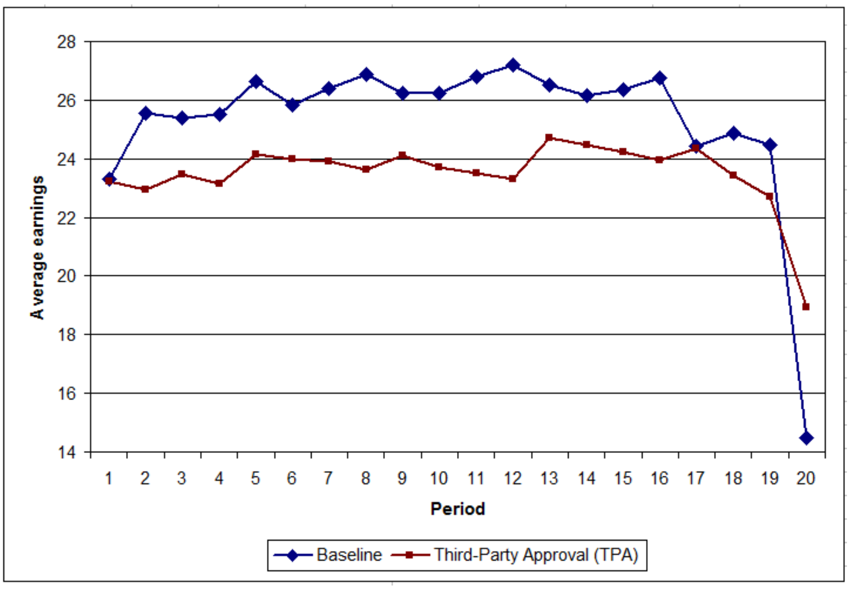


Figure 3.2: Average earnings over period by treatment

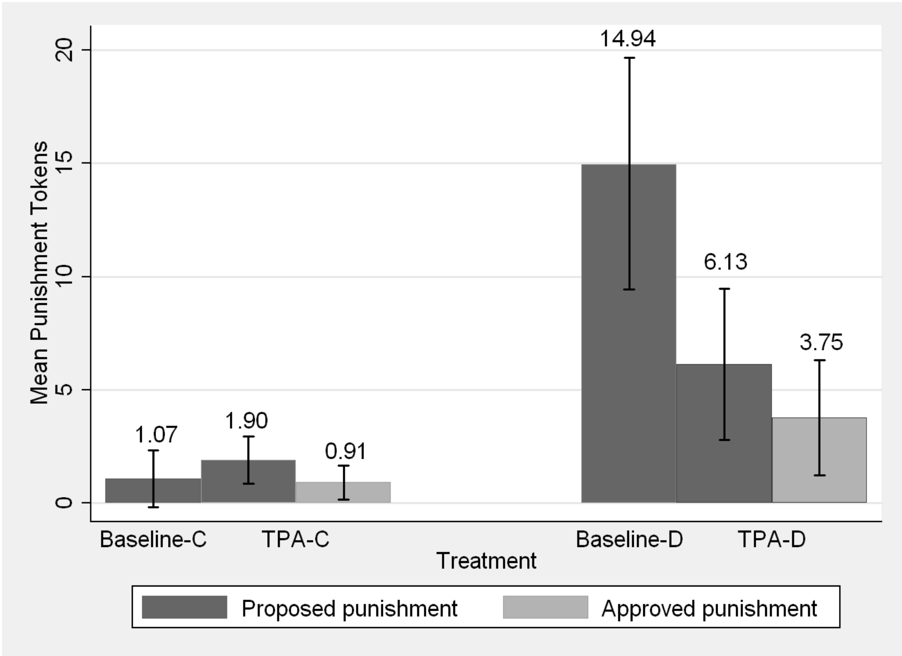


3.3.2 Punishment comparisons across treatments

To understand the observed treatment effect on cooperation, we test our hypotheses by comparing the punishment decisions across treatments. Given that punishment severity is determined by both the frequency and the amount, we report in Figure 3.3 the expected proposed and enforced punishment amount to the cooperators and defectors across treatments.

We find supporting evidence for Hypothesis 3.1 that third parties reject antisocial punishment. As shown in Figure 2, in the TPA treatment, C significantly reduces the implementation of antisocial punishment proposals (1.90 vs. 0.91, $p < 0.05$). The data also support Hypothesis 3.2 that third parties lower punishment of defectors (6.13 vs. 3.75, $p < 0.1$). Moreover, our data suggest an indirect effect of third parties' interventions on the punishment proposals especially to the defectors, which may also contribute to the low cooperation level in the TPA treatment. Compared to the baseline, A and B propose much less punishment on defectors in the TPA treatment, although the difference is not statistically significant (6.13 vs. 14.94, $p = 0.13$). The proposed antisocial punishment remains very low in the TPA treatment as in the baseline although it is marginally significantly higher than the baseline (1.90 vs. 1.07, $p < 0.1$).

Figure 3.3: Expected punishment amount by treatment



Notes: For each group in each treatment, we calculate the product of the average punishment towards cooperators/defectors and the frequencies of cooperation/defection decisions. The expected punishment amount is the average of these products. Baseline-C means antisocial punishment to cooperators in baseline; TPA-C represents antisocial punishment to cooperators in TPA; Baseline-D means punishment to defectors in baseline; TPA-D means punishment to defectors in TPA. In the baseline, the proposed punishment is also the enforced punishment.

Consistent with the previous studies, our data suggest that antisocial punishment decreases cooperation while punishment imposed on defectors increases cooperation. In both treatments, cooperators, if not punished, continue cooperating in about 90% of cases in the next period. In contrast, if punished, cooperators in both treatments continue cooperating in less than 50% of cases. Defectors, if not punished, cooperate in the next period in less than 10% of cases in both treatments. But when they are punished, they switch to cooperation in 22.58% of cases in the baseline and 35% of cases in the TPA treatment.

As we discussed above, our hypotheses indicate that the overall effect of the third party's intervention is determined by how often antisocial punishments occur. In our experiment, antisocial punishment occurred only in a few cases. In particular, cooperators are punished in 17 out of 1036 cases (1.64%) whereas defectors are punished in 80 out of 444 cases (18.02%) in the baseline. In the TPA treatment, cooperators are proposed to be punished in 54 out of 716 cases (7.54%) whereas defectors are proposed to be punished in 291 out of 804 cases (36.19%). As a result, the third party's intervention leads to a lower cooperation rate because it largely decreases the positive impact of punishment on defectors. Interestingly, the lower cooperation rate in the TPA in period 1 seems to suggest that subjects have anticipated the ineffectiveness of the mechanism.

Result 3.1 *Third parties lower both antisocial and normal punishment. Moreover, the presence of a third party lowers punishment proposals towards defectors, resulting in a decrease in cooperation and average earnings.*

3.3.3 Third-party decisions

Table 3.3 shows how Person C approves A and B's punishment decisions in each scenario.

Table 3.3: Third-party judgment decisions

Punishment scenario	Always reject	In Between	Always approve	Total
Cooperator punishes Defector	14.29%	23.81%	61.90%	100%
	(3)	(5)	(13)	(21)
Defector punishes Cooperator	64.71%	23.53%	11.76%	100%
	(11)	(4)	(2)	(17)
Defector punishes Defector	19.04%	66.67%	14.29%	100%
	(4)	(14)	(3)	(21)
Cooperator punishes Cooperator	33.33%	33.33%	33.33%	100%
	(1)	(1)	(1)	(3)

Notes: The upper number of a cell is the percentage of third parties approving a certain category of punishment proposal throughout the experiment. The number in the parenthesis is the absolute number. For instance, the number 14.29% means "14.29% (3) of all Person C's (12) who had to decide whether to punish the defector as proposed by his cooperator counterpart rejected punishment proposals."

We find that nearly 65% of all Person Cs always reject antisocial punishment proposals. This percentage is much higher than the rejection rate of the proposals targeting at defectors. On the contrary, more than 60% of Person Cs approve cooperators' decisions of punishing defectors during the experiment.

Table 3.4: Third-party judgment decisions

Rule	No. of third parties
Approves everything	16.129%
	(5)
Vetoes everything	16.129%
	(5)
Approves cooperators punishing defectors only	19.355%
	(6)
Vetoes defectors punishing cooperators only	16.129%
	(5)
Approves cooperators punishing defectors &	19.355%
Vetoes defectors punishing cooperators	(6)
Others	12.903%
	(4)
Total	100%
	(31)

Notes: "Approves everything" means that a third party permits every punishment proposal regardless of the actions of the punishers in the game and the periods of the proposals made. "Vetoes everything" means a third party shuts down every punishment proposal throughout the experiment. "Approves cooperators punishing defectors only" means that the only form of punishment a third party consistently approves is letting cooperators punish defectors. "Vetoes defectors punishing cooperators only" means that a third party only consistently disallows cooperators punished by defectors but judges inconsistently for other forms of punishment. "Approves cooperators punishing defectors & Vetoes defectors punishing cooperators" means that a third party vetoes antisocial and approves normal punishment consistently. "Others" means a third party does not make consistent judgment of any kind. Seven third-party judges never have the opportunity to make any decisions during the entire experiment and hence are excluded from the analysis.

Table 3.4 summarizes the strategies of Person Cs in approving punishment proposals. Note that we do not have complete strategies for every third-party judge since most of them do not have the opportunity to judge in all possible scenarios. Nonetheless, we find the criteria of third party judgment mostly consistent. Nearly 40% of them firmly agree that cooperators can punish defectors, although not so for the other cases. Around 35% consistently veto antisocial punishment proposals. Only less than 20% of all third parties are unpredictable when facing antisocial punishment proposals by defectors or normal ones by cooperators.

Figure 3.4: Relationship between proposed and approved punishment in TPA treatment

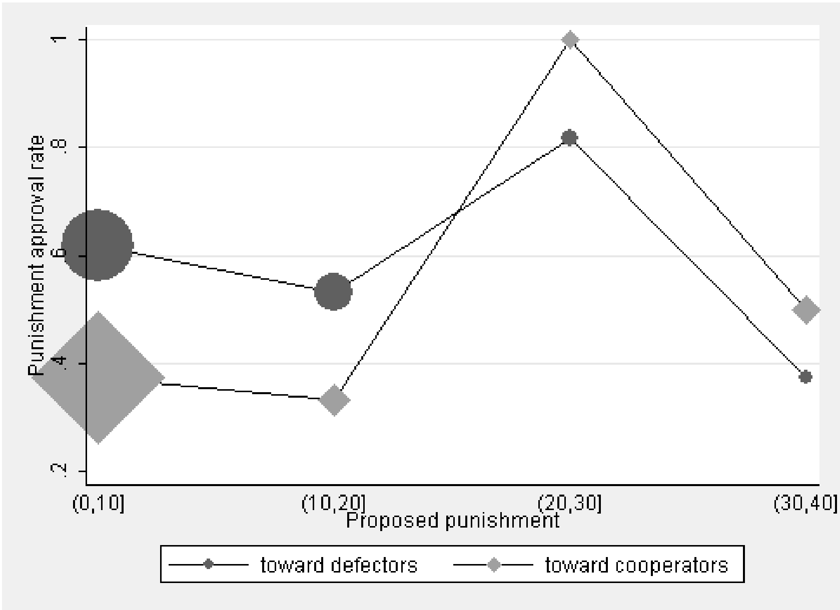


Figure 3.4 depicts the correlation between proposed punishment approval rates based on the nature of the proposals. The horizontal axis represents punishment proposals within a certain range (increment of 10). The vertical axis is the punishment approval frequencies. For instance, punishment proposals toward defectors which are less than 10 tokens are approved 61.9% of the time. The size of an observation stands for the number of observations. The larger a circle/diamond, the larger the number of observations of a certain category.

We find that over 80% of all punishment proposals are lower than 20 tokens regardless of the nature. Antisocial punishment proposals are rejected more frequently than normal ones towards defectors.[5] However, we do not detect a strong correlation between the number of punishment proposals and approval rate. It implies that the approval decisions of the third parties do not merely depend on punishment magnitudes but other factors.

To provide a quantitative analysis of how Person Cs approve punishment decisions, we conduct a probit regression analysis of Person C decisions in TPA treatment. Previous research indicates that third parties are willing to incur cost to punish norm violators (Fehr and Fischbacher (2004a), Ottone (2005) Xiao and Kunreuther (2010)). Hence, third parties in our setting should take into account who initiates the punish-

[5]The pattern seems to reverse for proposals larger than 20. However, this pattern is far from conclusive due to the lack of sufficient observations. For punishment proposals larger than 20, there are only 3 observations for antisocial proposals (5.55%) and 19 for normal ones (6.53%).

Table 3.5: Third-party judgment regression

	one cooperator and one defector	defectors only
β_1 Punishment	-0.831**	–
proposer is a defector	(0.386)	–
β_2 Positive	0.026	-0.186**
punishment proposal difference	(0.064)	(0.073)
β_3 Negative	-0.010	0.156
punishment proposal difference	(0.085)	(0.121)
β_4 The punished	0.133	-0.001
also proposes punishment	(0.343)	(0.314)
β_5 Period	0.013	0.014
	(0.030)	(0.015)
β_0 Constant	0.419	0.404
	(0.376)	(0.364)
Observations, individuals	84,26	237,21

Notes: * 10% significance level; ** 5% significance level; *** 1% significance level. We only exclude all the data when neither players propose punishment. A Likelihood-Ratio test rejects the null hypothesis that the third-party judgment decisions are statistically equivalent if only one player defects versus the case where both players defect. Hence we run two separate equations. A logit regression yields similar results.

ment and to whom the punishment is sent. Furthermore, third parties also care about distributional outcomes (Leibbrandt and Lopez-Perez (2010)), which means in our setting, how much the two proposals differ. We include all these factors as independent variables. We also control for a time effect, and take into account within-subjects correlation by letting standard errors cluster on third-party IDs.

Table 3.5 presents the results. Ceteris paribus, a third party is significantly less likely to approve a proposal if it is filed by a defector and targeting a cooperator (β_1). Controlling for that, however, punishment magnitude does not influence third-party decisions. Another interesting pattern is also evident in the table. If punishment proposals come from one defector to another defector, a third party is less likely to approve the one with larger punishment magnitude (β_2), but not significantly more likely to approve the one with smaller punishment magnitude (β_3). This finding seems to indicate that third parties are sensitive to relative punishment magnitude only when both players have identical actions and earnings in the game. In the cases where both players defect, nobody is more "right" or "wrong". Therefore, a third party may find it harder to justify a punishment proposal if it asks for a lot of deductions from the other party.

Result 3.2 *Third parties have consistent but heterogeneous judgment criteria on punishment proposals. Their judgment is affected by players' actions and relative proposal magnitudes.*

Table 3.6: Reaction to punishment across treatments

What happens at period $t-1$	Player i was a defector	Player i was a cooperator	Player i was a defector	Player i was a cooperator
β_1 PunAmtReceived	-0.889	-2.367***	-0.271	-2.267***
from a defector	(0.623)	(0.483)	(0.374)	(0.596)
β_2 PunAmtReceived	1.378***	5.243	-6.265	-1.872*
from a cooperator	(0.374)	(22.711)	(84.227)	(1.072)
β_3 Period	-0.082	-0.033**	-0.066***	-0.057***
	(0.023)	(0.015)	(0.018)	(0.017)
No. of observations	404	1002	745	699
individuals	42	66	58	60
Likelihood	-97.425	-188.610	-167.651	-199.065

Notes: 10% significance level; ** 5% significance level; *** 1% significance level.

Third parties are more likely to accept punishing defectors than cooperators. When both players defect, third parties are more likely to reject the player with larger punishment proposals.

3.3.4 Player reactions to punishment

One main reason why a punishment mechanism is effective in promoting cooperation is that defectors cooperate more after being punished. For example, many previous papers on social dilemma games find that low contributors on average respond to punishment by raising their contributions in the subsequent period (Masclet et al. (2003); Falk et al. (2005)). Table 3.6 reports players' reactions to punishment in both treatments. The coefficient β_1 measures the effect of the total punishment amount received from a defector on subject i's propensity to cooperate in period t, and β_2 is this effect if punishment cames from a cooperator. We control for the time effect (β_3) and individual unobserved characteristics.

The estimates in Table 3.6 show that in the baseline treatment, if player i is a defector, then there is a positive relation between the punishment points from a cooperator and the extent of player i's cooperation propensity (β_2). However, this effect is not significant in the TPA treatment. It is also worth noting that cooperators react negatively to punishment from defectors in both treatments (β_1).

Result 3.3 *Defectors in the TPA treatment are less responsive to punishment compared to the baseline treatment when they are punished by cooperators.*

3.4 Conclusion

We conducted experiments to study the impact of peer punishment on promoting cooperation when the punishment proposers are independent of enforcers.

Our experimental data suggest that independent punishment enforcers diminishes punishment both for cooperators and defectors. One implication is that when antisocial punishment is not pervasive, the intervention of a third party reduces the effectiveness of the peer punishment mechanism in promoting cooperation.

Our study provides the first evidence that third-party intervention could lower cooperation in social dilemmas although it also controls antisocial punishment. As a first step, we exclude many features of the punishment mechanism with third-party approval in the naturally occurring environment. For example, we randomly assign a subject to be a third party in the experiment. In reality, the right to the enforcer is either legalized by law (such as judges) or elected by the governed people (such as tribal leaders). Moreover, a third party often benefits from good reputations and cooperation amongst the parties involved. In future studies, we consider allowing a third party endogenously elected by players. It is also important to study how different empowerment of a third party affects judgement and cooperation. For instance, which third-party approval mechanism elicits the highest cooperation from the parties involved? If the third party can, say, double the proposed punishment amount, will that change the robustness of our main results?

Nevertheless, our findings draw attention to the importance of studying how these features influence effective punishment institutions when an independent third party is empowered with enforcement rights.

3.A Appendix

Subject instructions for TPA treatment

Instructions

Thank you for coming to the experiment. Please read these instructions carefully! Talking is not allowed at any time during this experiment. If you have a question, please raise your hand, and an experimenter will assist you.

This experiment consists of 20 periods. Each participant is in the role of either Person A, or Person B, or Person C. Each participant's role will be randomly determined by the computer at the beginning of the experiment and remain the same during the experiment. The computer will also randomly group a Person A with a Person B and a Person C at the beginning of the experiment. Each participant will stay in the same group during the whole experiment (i.e. 20 periods).

Each period consists of two stages as described below:

Stage 1: Person A and Person B will simultaneously and individually decide whether to choose "Option I" or "Option II". Each one's earnings are determined as follows: (a) if both Person A and Person B select Option I, each earns 30 tokens; (b) if both Person A and Person B select Option II, each earns 20 tokens; and (c) if one selects Option I and the other one selects Option II, the one who chooses Option I earns 15 tokens and the one who chooses Option II earns 40 tokens. The payoff table below lists all the possible payoff outcomes for each possible scenario. The number on the left in each cell is Person A's payoff and the number on the right is Person B's payoff.

Table 3.7: Payoff table

		Person B	
		Option I	Option II
Person A	Option I	30, 30	15, 40
	Option II	40,15	20,20

Person C's earnings in each period is determined by a random process. The computer will randomly assign 30, 15, 40 or 20, *with equal chance,* as Person C's earnings in the first stage of each period. Person C will not know this randomly assigned payoff amount until the end of the experiment. Neither Person A nor Person B will ever know Person C's randomly payoff throughout or after the experiment.

Stage 2 At the beginning of the second stage, all participants of a group will be informed of the decisions and earnings of the Person A and the Person B of the group. Again, Person C's earnings will not be revealed to anyone including Person C him/herself.

First, Person A will have an opportunity to individually propose to the Person C in his/her group whether to deduct any amount of tokens from the matched Person B's payoff. Every three tokens deducted from Person B's payoff, *if approved by Person C (see details below)*, will cost Person A one token. Meanwhile, Person B will also have the same opportunity to propose to Person C whether to deduct any amount of tokens from the matched Person A's payoff. Similarly, every three tokens deducted from Person A's payoff, if approved by Person C, will cost Person B one token.

The maximum deduction amount a person can impose on the other is 40 tokens. That is, you can propose to deduct from the other person's payoff by any amount of tokens between 0 and 40. However, the amount should be an integer number (e.g. 0, 1, 2. . .).

Next, Person C will see both Person A's and Person B's decisions and payoffs in the first stage, and also Person A's and Person B's proposals in the second stage.

- If either Person A or Person B proposed any deduction for the matched Person B or Person A, then Person C will have to decide whether to approve the proposal(s). If Person C decides to approve the proposed deduction amount, then the deduction will be implemented. In this case, the proposer's earnings will also be deducted by one-third of the deduction amount as we noted above. Person C's payoff will not change no matter what decision he/she makes. *(Note: Person C does not know his/her randomly assigned payoff in the first stage when making this decision).*

- If neither Person A nor Person B proposed any deduction, then Person C does not have any decision to make. No change will be made for any one's earnings.

At the end of each period, each participant will see the decisions of each one in his/her group, the final earnings of Person A and B in that period.

Each period will proceed in the same way. Each participant will play 20 periods with the same participants. Your final earnings are the sum of your earnings over 20 periods. After all the 20 periods finished, each participant will receive a cash payment in private. The exchange rate of tokens to Euro is:

40 tokens = 1 Euro

Examples

Below are some examples to illustrate how payoffs in each period are determined.

Suppose, in one period, in the first stage, Person A chose Option I and Person B chose Option II. Thus, Person A earns 15 tokens and Person B earns 40 tokens in the first stage. Suppose, in the second stage, Person A proposed to deduct 9 tokens from Person B's earnings and Person B proposed to deduct 6 tokens from Person A's earnings. Also, suppose the computer randomly assigns 30 tokens as Person C's earnings in that period.

- If Person C approved Person A's proposal but rejected Person B's proposal, each one's earnings in this period are as follows:
  - Person A's earnings $= 15 - 3 = 12$
  - Person B's earnings $= 40 - 9 = 31$
  - Person C's earnings $= 30$
- If Person C approved both proposals in the above scenario, then each one's earnings in this period are as follows:
  - Person A's earnings $= 15 - 3 - 6 = 6$
  - Person B's earnings $= 40 - 2 - 9 = 29$
  - Person C's earnings $= 30$
- If Person C rejected both proposals in the above scenario, then no one's earnings would be changed. Each one's earnings in this period are as follows:
  - Person A's earnings=15
  - Person B's earnings=40
  - Person C's earnings=30

Now suppose in the second stage of that period, Person A proposed to deduct 8 tokens from Person B's earnings and Person B did not propose any deduction amount. In this case, Person C only needs to decide whether to approve Person A's proposal.

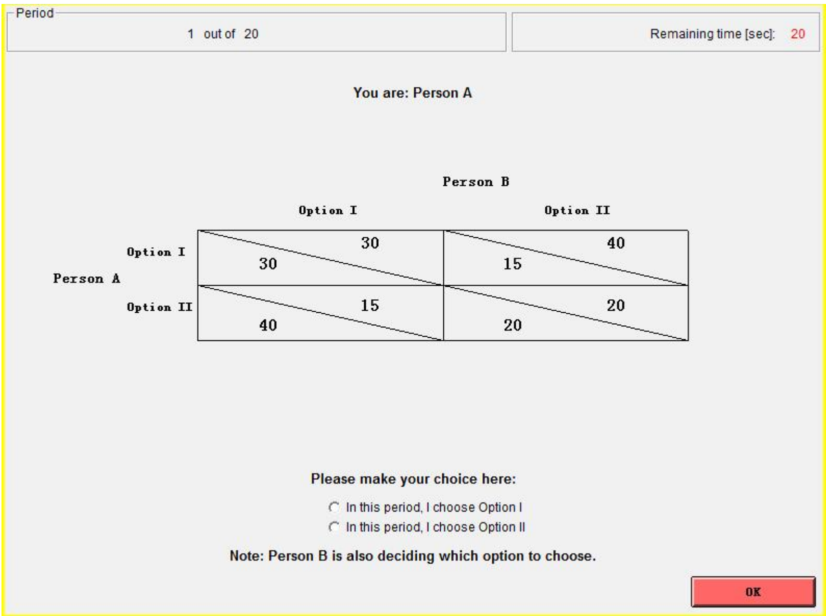
- If Person C approved it:
  - Person A's earnings $= 15 - 2.67 = 12.33$
  - Person B's earnings $= 40 - 8 = 32$
  - Person C's earnings $= 30$
- If Person C rejected it, then no one's earnings would be changed:
  - Person A's earnings=15
  - Person B's earnings=40
  - Person C's earnings=30

Summary
To repeat, each participant will play in the same group for 20 periods. Each period consists of two stages. In the first stage, each Person A and Person B will decide whether to choose Option I or Option II which will decide each one's earnings in the first stage. Person C's earnings in a period will be randomly determined by the computer (Person C will know this amount only at the end of the experiment after all the decisions have been made. Person A and Person B will not be informed about this amount throughout the experiment). In the second stage, after knowing Person A and Person B's decisions in the first stage, Person A (Person B) will decide whether to propose any deduction amount to the paired Person B (Person A). The proposed deduction amount will implemented only if Person C approves it. Person C's earnings will not change no matter whether s/he approves the proposal(s).

Please raise your hand if you have any questions at this moment.

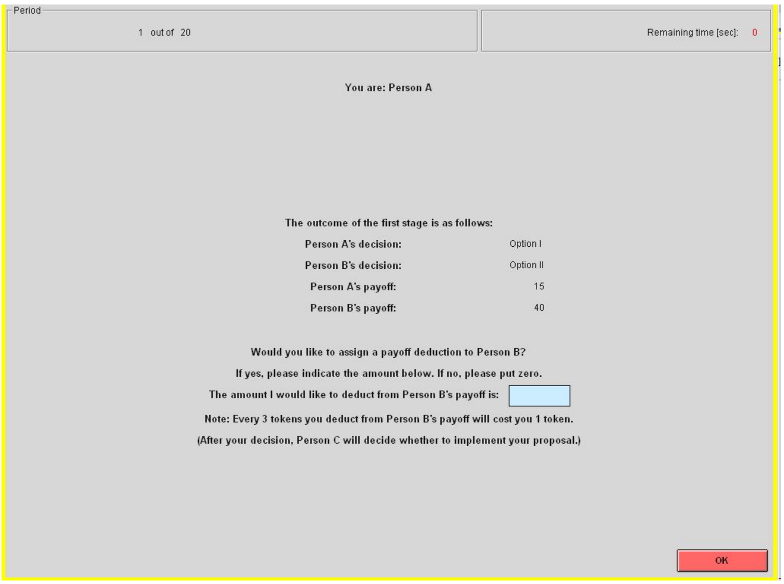
The next several pages outline the procedure of the experiment and the computer screens when Person A, Person B and Person C make their decisions.

In the first stage, Person A and Person B simultaneously decide which option to choose. Person C will be waiting.
SCREEN 1



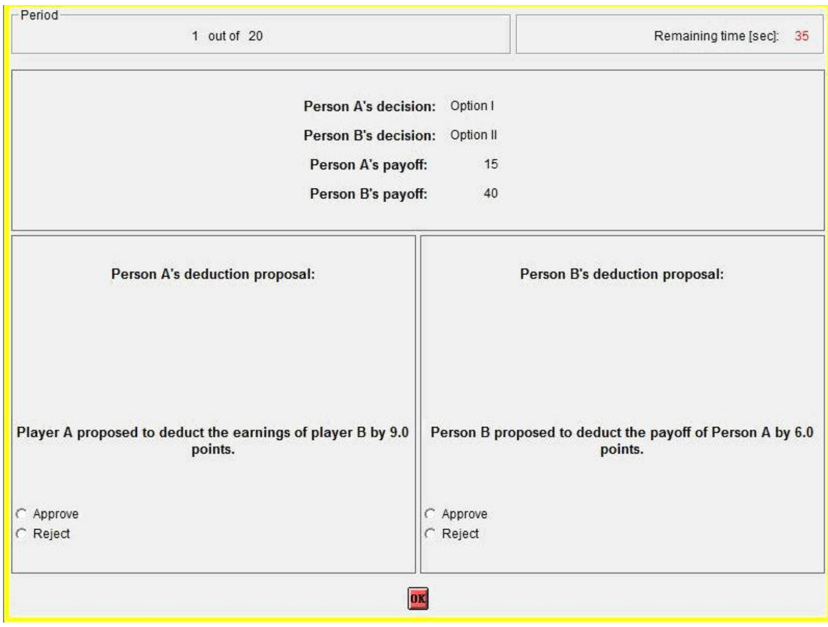
In the second stage, Person A and Person B receive feedback regarding the outcome of the first stage. Then each proposes to the matched Person C whether to deduct any amount of tokens from the matched Person A's or Person B's earnings.

SCREEN 2



Next, Person C will see both Person A's and Person B's decisions and payoffs in the first stage, and also Person A's and Person B's proposals in the second stage. Then, Person C decides whether to approve or reject the proposal if any. **Note: Once Person C clicked the "OK" button, s/he cannot change the decision.**

SCREEN 3



At the end of each period, each participant (Person A, B and C) will see the outcome of that period.

SCREEN 4


Period
1 out of 20
Remaining time [sec]: 2
You are: Person A
The outcome of this period is as follows:
Person A's decision: Option I
Person B's decision: Option II
Person A's payoff in the first stage: 15
Person B's payoff in the first stage: 40
The amount Person A deducted from Person B's payoff: 9.0
The amount Person B deducted from Person A's payoff: 6.0
Person C's decision on Person A's proposal: Approve
Person C's decision on Person B's proposal: Approve
Person A's final earnings in this period: 6.0
Person B's final earnings in this period: 29.0
OK

CHAPTER 4

GROUPS VERSUS INDIVIDUAL PLAY IN SEQUENTIAL MARKET GAME [1]

4.1 Introduction

Many decisions in private, public, and business life are not taken by individuals, but by groups of individuals. Think, for instance, of households, public authorities, court juries, boards of directors, or management teams.[2] Recently growing experimental research on interindividual-intergroup comparisons in controlled laboratory experiments suggest that groups differ from individuals from many aspects. Hence, in the presence of systematic differences in decisions made by individuals and groups, it would be risky to export results observed in interindividual decision making to domains were groups interact with each other (see, e.g., Cooper and Kagel (2005)).

Recently growing experimental research on interindividual-intergroup compar-

[1]This chapter is joint work with Wieland Müller. We are grateful to David Vonka for technical consulting and help. We thank Marco Castillo, Guillaume Fréchette, Daniel Houser, Rudolf Kerschbamer, Charles Noussair, Ragan Petrie, Jan Potters, Andrew Schotter, Matthias Sutter, Jean-Robert Tyran and seminar participants at Innsbruck University, Tilburg University, New York University, George Mason University, the 3rd Maastricht Behavioral and Experimental Economics Symposium, and the ESA world meeting 2010 in Copenhagen, and the Symposium on Industrial Organization and Management Strategy 2011 in Chengdu for helpful comments. Furhermore, we thank James C. Cox, Daniel Friedman, and Steven Gjerstad as well as Sau-Him Paul Lau and Felix Leung for making available estimation codes. Wieland Müller acknowledges financial support from the Netherlands Organisation for Scientific Research (NWO) through a VIDI grant.

[2]For example, the chairman's office of the News Corporation is a group of five persons meeting every week to consider "every acquisition and item of capital expenditure" (FT May 20th, 2003). More generally, the organization literature has long researched the roles of management teams in firms. As Finkelstein and Hambrick (1996) point out, decision makers are informed, influenced and sometimes constrained by others, both inside and outside the organization.

isons has so far derived the result that, indeed, often there are differences in the behavior of individuals and groups. More precisely, although there are exceptions, one result that emerges from the literature is that often groups appear to be more selfish than individuals. This has mainly been shown in the context of two classes of games. The first class consists of simple, sequential-move, two-player games such as the ultimatum game (Bornstein and Yaniv (1998), and Robert and Carnevale (1997)), the trust game (Cox (2002), and Kugler et al. (2007)), the centipede game (Bornstein et al. (2004)), and the gift-exchange game (Kocher and Sutter (2007)). [3] Bornstein (2008) (p. 30) summarizes much of this literature by stating that:

> "Groups, it seems, are more selfish and more sophisticated players than individuals, and, as a result, interactions between two unitary groups are closer to the rational, game-theoretical solution than interactions between two individuals."

Note that the literature Bornstein summarizes in this quote is based on experimental games in which individuals and groups interact only once. The second class consists of games that authors characterize as having a "Eureka" component, meaning that once the solution or equilibrium is found, it is recognized as a clear solution of the game. Based on results from, e.g., signaling games (Cooper and Kagel (2005) and beauty contests (Kocher and Sutter (2005), Sutter et al. (2009) (p. 391) state that

> "It can be considered a stylized fact in the literature that teams are generally closer to game-theoretic predictions than individuals in (interactive) games in which rationality and correct reasoning are the predominant task characteristics."

Moreover, to the extent that groups and individuals converge to the same equilibrium in these repeated "Eureka"-type games, groups are found to do so much faster than individuals.

In this paper we contribute to the literature on interindividual-intergroup comparisons by studying a Stackelberg market game which, arguably, belongs to the first class of games above. A particular aim is to study the effect the time horizon of interaction has on the behavior of individuals and groups - a topic that has not yet been thoroughly studied in this class of games. Our results are in (partial) contrast to the quotes above. In fact, in our one-shot Stackelberg markets we find no significant differences in the behavior of groups and individuals, and in our repeated Stackelberg markets we find that the behavior of groups is *further away* from the subgame-perfect equilibrium than that of individuals. That is, we show that once a simple sequential-move game (belonging to the class of games summarized by Bornstein et al. (2008))

[3] One exception is provided by Cason and Mui (1997) dictator games where, in some cases, group dictators give more than individual dictators. In their re-examination, Luhan et al. (2009) team dictators to be more selfish than individual dictators.

is repeated, the behavior of groups relative to that of individuals goes in the opposite direction to what is stated in Bornstein's summary. In particular, group play *diverges* from the (refined) game-theoretic solution.

The Stackelberg (1934) model is among the most frequently applied models of oligopolistic competition. In a Stackelberg duopoly market game, one firm (the first mover) makes its quantity decision first. Then, knowing the first mover's choice, the other firm (the second mover) decides on its quantity, before the market clears. In case of linear market demand and symmetric and constant marginal costs, in the subgame perfect equilibrium the first mover produces and earns twice as much as the second mover. Moreover, the second mover's best response is a linear and downward sloping function of the leader's quantity choice.[4] We chose a Stackelberg game because it has a very attractive feature: For each of the first mover's quantity choice, a second mover can, by its own quantity choice, express a wide range of preferences over own and the other player's income.[5]

We implement this market game by having either individuals or groups of three subjects act in the role of the first and the second mover. Subjects acting in groups have to unanimously agree on the quantity being produced. The decision making process within groups is aided by access to a chat tool. The members of a group are able to exchange written messages until they reach a joint decision.

Comparing first mover quantities across treatments is straightforward. In the one-period games we find that although the average group leader quantity is somewhat higher than the average individual leader quantity, the difference is insignificant. In the multiple-period games, in contrast, we find that average leader quantities chosen by groups are significantly lower than average leader quantities chosen by individuals. Comparing second mover behavior across treatments is less straightforward as we observe followers' choices in response to varying first mover choices. Nevertheless, for the one-period games we find that, if anything, the observed average response function of groups is closer to the best-response function than that of individuals, which is in line with earlier experimental results. But, again, we fail to detect statistical differences. In the multiple-period game treatments, average observed reaction functions of followers display a specific non-monotonic pattern not predicted by standard theory. However, this pattern is predicted and can be accounted for by models of other-regarding preferences. We use maximum-likelihood techniques to estimate average follower response functions for the multiple-period treatments, using either Lau and Leung (2010) implementation of the Fehr and Schmidt (1999) model of inequality aversion or the Cox et al. (2007) model of emotion-driven reciprocity. As the standard best response function of followers is nested in both of these mod-

[4] Experimental evidence on individual-player Stackelberg duopoly markets and how they compare to simultaneous-move Cournot duopoly markets is reported in Huck et al. (2001).

[5] This feature distinguishes the Stackelberg game from other sequantial games such as the ultimatum game or the trust game.

els, we have a clear and unambiguous method to test which of two observed average response functions is closer to the prediction of subgame perfectness. Irrespective of which of the two models we use to account for followers' reaction functions, we find that the one employed by groups is further away from the standard best response function than that of individuals.

Since individuals and groups partly choose markedly different quantities as first movers, differences we observe in individual and group second-mover decisions might be driven by different experiences second movers make in the individual and the relevant group-player treatments. We control for this by also eliciting choices in four additional treatments employing the strategy method (Selten (2008)) in which, simultaneously with the first movers making their decision, the second movers have to indicate how they would react to each of the first movers' quantities. Thus, this method gives us the complete response function of second movers. The results of the control treatments largely confirm the results obtained in the main treatments with truly sequential play. In the one-shot sessions, behavior appears to be in line with results reported in the literature as group leaders and followers are closer to the prediction of subgame perfectness, although the differences are insignificant. In the multiple-period treatments, we find, again, that in comparison to individuals, groups choose lower leader quantities and employ response functions that are further away from the standard best response function.

Our paper makes two main contributions. The literature reports so far that in simple, two-player, sequential-move games groups often appear to be *closer* to the game-theoretic prediction than individuals if the game is played only once. We show for a game belonging to this class of games that once the game is repeated, the result is turned around in the sense that groups are shown to be *further away* from the game-theoretic prediction. The Stackelberg market game is, arguably, not a "Eureka"-type problem that has a clear solution, which, once found, is clearly seen as such by players. Instead, a Stackelberg duopoly market is a game that, as the other games summarized by Bornstein et al. (2008), leave more room for otherregarding preferences. In these games, the presence of profit-maximizing and otherregarding motives might play out differently depending on whether the game is played by groups or by individuals and depending on the time horizon of interaction. In fact, to explain our results, in the discussion section we provide evidence that there is heterogeneity in subjects' types. Concentrating on second movers, we find that they are often either myopic profit maximizers (who always best respond to a first mover's quantity), strategic rewarders and punishers, or preference-driven rewarders and punishers. [6] The latter two types' behavior is indistinguishable until the last period (until which both types employ a reward-and-punishment scheme). In the last period, however, strategic punishers and rewarders play best-response, while preference-driven pun-

[6] This categorization is reminiscent of types in public-good games identified in Luhan et al. (2001) or, more recently, Reuben and Suetens (2009)).

ishers and rewarders continue to employ a reward-and-punishment scheme. Subjects of these varying types play largely uninfluenced by each other in the individual treatments but influence each other via group discussions in the group treatments. We illustrate how this can lead to different results depending on the different time horizons adopted in our and earlier experiments. Our results suggest that the apparent consensus in the literature regarding sequential two-player games, as summarized by the Bornstein (2008) quote above, needs to be modified to accommodate for differential effects of the time horizon of interaction and possibly other design features - a point we discuss in more detail in the concluding section. In any case, the answer to the question of who behaves more like a game theorist, groups or individuals, is not independent of the time horizon of interaction.

Our second main contribution is on a methodological level. We run both one-period and multiple-period games and employ the strategy method for the first time in a "group" experiment and in a repeated Stackelberg market game.[7] Doing so not only enables us to control for different first-mover actions across treatments, but also to uncover the shape of complete response functions in (repeated) individual and group Stackelberg markets. The heterogeneity in followers' behavior mentioned above implies that average response functions in both the individual and the team treatments show a somewhat surprising pattern: they slope downward for low leader quantities, slope upward for intermediate leader quantities (around the Cournot quantity), and slope downward again for higher leader quantities. This results suggests that it is not justified to account for response functions in e.g. sequential market games by running simple linear regressions. As other authors and we demonstrate, structural estimation of other-regarding preference models are able to account for the shape of average and complete individual response functions and are, thus, theory-driven alternatives to account for follower behavior.[8]

The remainder of the chapter is organized as follows. Section 4.2 gives a brief overview of the related literature, concentrating mainly on the earlier studies of interindividual and intergroup decision making in sequential two-player games. Section 4.3 introduces the experimental design and the main hypotheses. In Section 4.4 we report our results and present the estimations of structural models accounting for second-mover behavior. In Section 4.5 we discuss our results and Section 4.6 provides a summary and offers some concluding remarks.

[7]Huck and Wallace (2002) elicit complete-response functions in a one-shot Stackelberg experiment. However, we will show that the behavior these authors elicit (basically, a linear and downward sloping reaction function) does not constitute behavior of subjects who are given the opportunity to learn over the course of various rounds of play.

[8]Note that observed behavior is in line with that predicted by social-prefernce models, despite the fact that we use non-neutral "firm" language in the instructions and employ random-matching in the multiple-period treatments to weaken, to the extent possible, other-regarding motives.

4.2 Related literature

There is now a considerable number of studies comparing behavior of individuals and groups in experimental games. We mainly confine our overview to the papers most relevant for our purposes, that is, to sequential two-player games and market games. Doing so, we only very briefly describe the main results of these studies, relegating design details of the most relevant studies to Table 4.9 in the Appendix 4.A. Bornstein (2008) and Engel (2010) provide more complete overviews of the experimental literature on the behavior of groups.

The early studies on group decision making literature focus on ultimatum game. Bornstein and Yaniv (1998) find that groups in the role of the proposer offer less than individuals, and groups in the role of the responder showed a willingness to accept less. Robert and Carnevale (1997) also analyzed an ultimatum game, in which, however, no responders were present. These authors find similar results as Bornstein and Yaniv (1998) with respect to proposers.

Subsequent studies replicate this finding in other games. Cox (2002) analyzes an trust game (Berg et al. (1995) and reports no differences between groups and individuals playing in the role of the trustor. However, groups in the role of the trustee are reported to return significantly less than individuals. Kugler et al. (2007), on the other hand, find that groups are less trusting than individuals, but just as trustworthy. However, if there are differences, both studies point in the direction of more selfish behavior on the part of groups. Kocher and Sutter (2007) conduct a gift-exchange game and find that groups acting in the role of the employer and that of the employee chose lower wages and, in return, lower effort levels, respectively, than individuals. Bornstein et al. (2004) have both individuals and groups play two centipede games and report that groups exit the game significantly earlier than individuals. One exception is provided by Cason and Mui (1997) in a dictator game. They discover that in some cases, group dictators give more than individual dictators. However, a recent re-examination by Luhan et al. (2009) indicates that group dictators are more selfish than individuals, possibly caused by replacing the face-to-face discussion among group members with electronic chat. Bosman et al. (2006) study a power-to-take game where first movers can claim any part of the second movers' income. Then, second movers decide how much of the income to destroy. The authors do not find any differences between groups and individuals both in terms of the first-mover take rates and the income destroyed.

Some studies compare the behavior of groups and individuals in more complex IO settings. Bornstein et al. (2008), building on work by Bornstein and Gneezy (2002), analyze Bertrand price competition between individuals and between groups. They find that winning prices were significantly lower in competition between two- or three-person groups than in competition between individuals. In contrast to the results of Bornstein et al. (2008), Raab and Schipper (2009) find no differences in behavior by individuals or groups in Cournot competition. Nonetheless, this finding is still consistent with the previous literature in that individual players have already closely followed the Cournot equilibrium. Note, however, that earlier studies show that the Nash equilibrium is a good predictor in individual-player Cournot markets (see, e.g., Huck et al. (2004)). Cooper and Kagel (2005) analyze limit-pricing games (Milgrom and Roberts (1982)) and report that teams consistently play more strategically and learn faster than individuals. Similar finding is reported in Kocher and Sutter (2005) via a beauty-contest game. Feri et al. (2010) report that groups can coordinate more efficiently than individuals.

In sum, it seems fair to say that most studies that find differences in interindividual and intergroup comparison find that groups tend to behave more in line with game theoretic predictions, appear more selfish, and show less regard for others, leading Bornstein et al. (2008) and Sutter et al. (2009) to the summaries stated in the Introduction.

4.3 Experimental design, procedures, and hypotheses

4.3.1 The Stackelberg duopoly game and its predictions

In our Stackelberg duopoly game, two firms face an identical inverse demand function $p = \max\{30 - Q, 0\}$ with $Q = q_L + q_F$. Both players have constant unit costs of $c = 6$ and no fixed cost. Firms choose their quantities sequentially. First, the Stackelberg leader (L) decides on its quantity q_L, then, knowing q_L, the Stackelberg follower (F) decides on its quantity q_F. The subgame perfect equilibrium is given by $q_L = 12$ and the follower's best-reply function $q_F(q_L) = 12 - 0.5q_L$, yielding $q_F = 6$ in equilibrium. Joint profits are maximized if $q_L + q_F = 12$ and the Nash equilibrium of the simultaneous-move game (Cournot market) predicts $q_L = q_F = 8$.

The noteworthy feature of the Stackelberg game is that the slope of the payoff function is flatter for followers than for the leaders around the Stackelberg outcome (12,6).[9] That means a slight deviation from the Stackelberg follower quantity 6 will cause larger differences in profits for the leaders than for the followers. Hence, the slopes of the payoff function around the Stackelberg outcome creates sufficient incentives for followers to reward or punish leaders.

The following two motivations lead us to choose a Stackelberg game. First, in contrast to other sequential two-player games, a second mover in a Stackelberg game has a much richer strategy space. For instance, in an ultimatum game, the choice set of the responder is a binary set containing just two alternative, "accept" and "reject" . In

[9] The payoff function for the follower is $(24 - q_F - q_L)q_F$. Taking the derivate with respect to q_F becomes $24 - 2q_F - qL$. The payoff function for the leader is $(24 - q_F - q_L)q_L$. Taking the derivate with respect to q_F becomes -q_L. Hence, in the neighborhood of the Stackelberg equilibrium (12,6), the slope of the leader profit with respect to the changes of q_F will be steeper than that of the slope of the follower profit.

contrast, a second mover in a Stackelberg game has much more room to reciprocate a leader's action, both positively and negatively. As Cox et al. (2008) (p. 33) point out "The [Stackelberg] duopoly games are especially useful because the follower's opportunity sets [...] have a parabolic space that enables the follower to reveal a wide range of positive and negative trade-offs between her own income and the leader's income." The second motivation concerns potential results. Huck et al. (2001) find in their individual-player Stackelberg games, that, on average, first movers produce less and second movers produce more than predicted by theory. Hence, there is room for groups to be closer or farther away from the subgame-perfect equilibrium prediction than individuals.

4.3.2 Treatment design

Table 4.1: Experimental design

Treatment Name	Sequential Method	Strategy Method	Individual Players	No. Players	No. Periods	No. Subjects	Groups
"SEQ-IND-1"	Yes	No	Yes	No	1	18	9
"SEQ-TEAM-1"	Yes	No	No	Yes	1	36	6
"SM-IND-1"	No	Yes	Yes	No	1	18	9
"SM-TEAM-1"	No	Yes	No	Yes	1	36	6
"SEQ-IND-15"	Yes	No	Yes	No	15	36	6
"SEQ-TEAM-15"	Yes	No	No	Yes	15	72	4
"SM-IND-15"	No	Yes	Yes	No	15	36	6
"SM-TEAM-15"	No	Yes	No	Yes	15	72	4

Our experiment is based on a 2×2×2 factorial design, varying the number of periods of interaction (1 period or 15 periods), varying who acts in the two player positions of the Stackelberg game (individuals or groups), and varying the method of eliciting choices (truly sequential play or strategy method). We refer to the eight treatments as follows. The one-shot individual and group treatments with truly sequential play are called "SEQ-IND-1" and "SEQ-TEAM-1", while the one-shot individual and group treatments which employ the strategy method are called "SM-IND-1" and "SM-TEAM-1". The corresponding multiple-period treatments are, respectively, called, "SEQ-IND-15", "SEQ-TEAM-15", "SM-IND-15", and "SM-TEAM-15". Table 4.1 gives an overview of the design. Information about profits was given in the form of a payoff table (see Table 4.11 in the Appendix). Next, we describe the setting in each of the four treatments in detail.

Treatment SEQ-IND: This is a baseline treatment which is similar to the Stackelberg experiment in Huck et al. (2001). In each period, the first mover chose a quantity (selected a row in the payoff table). Knowing the quantity chosen by the first mover, the second mover then decided about his own quantity (selected a column in the table).

Treatment SEQ-TEAM: This is the team baseline treatment which was, with respect to timing, identical to SEQ-IND except that players were teams (consisting of three participants each) instead of individuals. To reach a joint decision, members of a team could exchange messages within a team via an electronic chat box.[10] There was no restriction regarding the contents of messages sent, except that (a) the discussion must be in English; (b) the language used should be civil and (c) subjects cannot identify themselves by revealing their names, seat numbers, etc. Subjects would enter their quantity decisions into a box in the decision screen and were then able to submit them to the other group members. All submitted quantity decisions of own group members then appeared on the screen of each group member. As long as not all submitted quantity decisions were the same, the chat box remained open and group members could continue discussing their decision. When all submitted quantity decisions of a team were the same, the decision screen (including the chat box) disappeared and subjects had to wait until the experiment continued.[11]

Treatment SM-IND: In this treatment, individual first and second movers made decisions according to the strategy method. That is, while first movers decided about a single quantity, second movers were, at the same time, asked to make a quantity decision for each of the 13 possible quantities the first mover could choose. When all subjects had made their decisions, the computer randomly matched first movers and second movers, and selected the relevant quantity of the second mover (that is, the quantity the second mover chose for the quantity chosen by the first mover).

Treatment SM-TEAM: This treatment is similar to treatment SM-IND, except that players are groups instead of individuals. The same communication technology as in treatment SEQ-TEAM was employed to facilitate group decisions. In particular, each member of a second-mover group had to indicate an entire strategy consisting of how it would react to each of the 13 possible choices of a first-mover team. At any point in the process of entering this strategy, second-mover group members could submit their strategy (entered so far) to the other group members. Similar to the individual-player treatments, all entered quantities submitted so far appeared on the screen of each group member. There were no restrictions in place regarding the order in which

[10]Electronic chat keeps the anonymity among subjects. As this paper is about studying how groups differ from individuals, it is best to use electronic chat in order to exclude the possible influences from other attributes (say, physical attractiveness) on communication.

[11]Our design follows the previous experimental studies on intergroup-interindividual decision making studies. Note that the focus of these studies is “how groups behave differently from individuals”, instead of “through which channel do groups behave differently from individuals - communication among team members, or mechanisms in aggregating their preferences”? One design to answer the latter question, we need an additional treatment in which subjects can talk to each other in the same role but then make independent decisions. Alternatively, one could exogenously impose various voting mechanisms and compare how they change the joint decisions of groups.

follower quantities for the 13 possible first-mover choices had to be entered on the decision screen. Again, the chat box remained open as long as group members had not yet entered the same complete strategy.

4.3.3 Experiment procedures

The experiment with 18 sessions was conducted at CentER Lab of Tilburg University in April, May, October 2009, and September 2010. Each session consisted of 18 subjects. A total number of 324 Tilburg University students participated in the study. Each subject took part in only one session. Each session consisted of either 1 period or 15 periods. In the repeated sessions, all 15 periods of play counted toward final earnings. There were no practice periods at the beginning of any session. On average, a one-shot session lasted about 45 minutes, whereas the repeated sessions lasted about 1 hour and 45 minutes (including the time to read the instructions and payment of the subjects). On average, a subject in a one-shot (repeated) session earned €7.29 (€18.51). The experiment was programmed and conducted with the z-Tree software (Fischbacher (2007)).

At the beginning of each session, subjects were randomly assigned to be either a first or a second mover, and these roles remained fixed throughout the entire session. In the team treatments, a team was formed by three players[12] who belonged to the same team for the entire experiment. Hence, a team-treatment session consisted of three first-mover teams and three second-mover teams. First-mover and second-mover teams were randomly rematched with each other in each of the 15 periods of the experiment. In order to control for the size of the random matching group, the 18 subjects in an individual-player session were divided into three cohorts of six subjects (three of which were first and they other three were second movers).

The instructions use non-neutral language, referring, e.g., to "firms," "product," or "profits". With the instructions, subjects received a payoff table (see the Appendix) which, to ease comparison, was the same as used in Huck et al. (2001). The payoff table showed all possible combinations of quantity choices and the corresponding profits. The numbers given in the payoff table were measured in a fictitious currency unit called "Points". Each firm could choose a quantity from the set 3,4,...,15. The payoff table was generated according to the demand and cost functions given above. [13] In each period, each individual first- or second-mover earned the amount indicated in the table for the selected quantity combination of both firms. In the team treatments,

[12]There is a large body of social psychology literature on the size of a small groups. The majority of them stipulate that the lower bound should be three people, for "a dyad (that is, two persons) is a much simpler social system" (see Fisher (1980).

[13]Due to the discreteness of the strategy space, such a payoff table typically induces multiple equilibria (see Holt, 1985). To avoid this, the bi–matrix representing the payoff table was slightly manipulated. By subtracting one Point in 14 of the 169 entries we ensured uniqueness of both the Cournot–Nash equilibrium and the subgame perfect Stackelberg equilibrium.

each member of a first- or second-mover firm also earned the amount indicated in the table for the selected quantity combination of both firms.

In the 15-period treatments, first and second movers (individuals or teams) were randomly rematched with each other in each period.[14] In the repeated game treatments, starting from the second period subjects were informed about the results of the previous round in their own market, including the quantity of the first mover, the (relevant) quantity of the second mover, and own profits.

4.3.4 Hypotheses

Recall that the Stackelberg market game has a unique subgame perfect equilibrium. Hence, the unique subgame perfect equilibrium of a repeated Stackelberg market game is to play the unique subgame perfect equilibrium of the stage game in each period of interaction. This implies that the selfish behavior in each period is described by the subgame perfect equilibrium of the stage game, even if our subjects in the 15-period treatments viewed the experiment as a finitely repeated game, despite the fact that we employed random-matching across periods. [15] However, in the experimental economics literature it is known that play in finitely repeated interactions might be more cooperative even if the stage-game equilibrium is unique and subjects are randomly rematched across rounds within relatively small groups (see, e.g., Selten and Stoecker (1986), or Andreoni and Miller (1993). Yet, in repeated interactions it is a priori not clear how groups would behave in comparison to individuals. Will groups have a tendency towards more selfish behavior in comparison to interindividual interaction as suggested by the earlier literature reviewed in Section 4.2? Or will there be a trend towards more cooperation in intergroup interaction as this, in the long run, would promise higher profits? The few studies reported in the economics literature find that groups in repeated interactions play more strategically and converge faster to the stage game equilibrium than individuals (Cooper and Kagel (2005) and Kocher and Sutter (2005)). Hence, based on these earlier results and those reviewed in Section 4.2, we should expect groups to be behave more in accordance with the prediction of subgame perfectness than individuals in both the one-period and the multiple-period treatments. More precisely:

Hypothesis 4.1 *Group first movers will choose quantities closer to the Stackelberg leader*

[14]One might argue that the sequency of one-shot games does not behave like a real one-shot game, and therefore the desired treatment condition is not completely created. Nonetheless, random matching across repetitions was also employed in the team versus individual play signaling games reported in Cooper and Kagel (2005). Note that, given the choice of a multiple-period treatment, random matching across periods constitutes a minimal change compared to a one-shot treatment. It is left for further research to analyze the effect of fixed matching across periods on interindividual and intergroup comparison in our Stackelberg market game.

[15]Random matching across repetitions was also employed in the team versus individual play signaling games reported in Cooper and Kagel (2005).

quantity than individual first movers, and group second movers' response functions will be closer to the standard best response function than that of individual second movers, independent of the duration of the interaction.

4.4 Experimental results

We report the results in two sections with the purpose of comparing behavior of individuals and groups in related treatments. The first section briefly presents summary statistics of our treatments, formal tests for differences in first mover behavior, and visual evidence of second mover behavior. In the second section we concentrate exclusively on second-mover behavior in the 15-period treatments, as accounting for it and formally testing for differences across treatments is much less straightforward than in the case of first movers. In fact, to account for the observed non-monotonic second-mover behavior, we are lead to estimate two social preference models: the (simplified) inequality-aversion model by Fehr and Schmidt (1999) as put forward by Lau and Leung (2010) and the parametric model of emotion-driven reciprocity by Cox et al. (2007). To purge the data of learning effects at the beginning of the 15-period sessions (especially in the strategy-method treatments) and, at the same time, preserve sufficient power for maximum-likelihood estimations, in the results section we report and use data from periods 3-15, if not otherwise indicated.

4.4.1 A first look at the data

Table 4.2 presents summary statistics of average quantity choices and payoffs for each treatment. The results of the 1-period (15-period) treatments are presented in the upper (lower) half of this Table. For the strategy-method treatments, only the relevant quantities of the second movers are taken into account (i.e., only quantity choices of second movers at quantities actually chosen by first movers).

In all treatments, we note that average first-mover quantities are clearly smaller and average second-mover quantities clearly larger than the predictions along the subgame perfect equilibrium path, which predicts quantity 12 for first and quantity 6 for second movers. To facilitate comparison, note that the average first (second) mover quantity observed in the 10-period random-matching Stackelberg game of Huck et al. (2001) was 10.19 (8.32). Hence, average quantities of 10.40 (7.78) chosen in our treatment Seq-Ind-15 (which comes closest in terms of design features to this earlier study) are similar to those reported in Huck et al. (2001).

Table 4.2: Summary of experimental results: Average quantities and payoffs

	Prediction		Truly Sequential Play				Strategy Method			
			SEQ-IND		SEQ-TEAM		SM-IND		SM-TEAM	
	Leader	Follower	Leader	Follower	Leader	Follower	Leader	Follower	Leader	Follower
					1-period treatments					
Individual	12	6	9.11	8.11	9.33	7.67	9.67	7.11	10.67	7.00
Quantities		(2.32)	(2.09)	(2.42)	(1.03)	(3.00)	(1.36)	(2.07)	(1.10)	
Total	18		17.22		17.00		16.78		17.67	
Quantities			(1.86)		(1.79)		(1.64)		(1.51)	
Individual	72	36	59.78	53.89	62.00	54.33	65.44	49.56	69.33	45.33
Payoffs			(18.19)		(20.09)		(18.86)		(10.33)	
					15-period treatments					
Individual	12	6	10.40	7.78	8.27	8.01	9.35	7.88	8.64	8.22
Quantities			(0.32)	(0.96)	(0.14)	(0.12)	(0.45)	(0.15)	(0.30)	(0.32)
Total	18		18.14		16.4		17.28		16.89	
Quantities			(0.90)		(0.90)		(0.29)		(1.17)	
Individual	72	36	57.59	43.42	60.54	59.23	57.90	51.16	59.09	55.97
Payoffs			(18.64)	(17.67)	(11.29)	(15.29)	(19.60)	(22.23)	(15.06)	(16.45)
Total	108		100.91		119.77		109.06		115.06	
Payoffs			(6.89)		(2.27)		(1.17)		(3.76)	

Notes: Standard deviations in parentheses.

Table 4.3 presents the statistical tests of the summary statistics. Concentrating on leader quantity, we find that first movers in treatment SEQ-IND choose significantly higher quantities than first movers in the corresponding team treatment SEQ-TEAM. This contradicts our main hypothesis.

Concerning follower quantities, it unambiguously appears that individual second movers behave more "selfishly" than team second movers, as the slope of the response function employed in the individual-player treatment SEQ-IND are significantly closer to the ones of the standard best-response function than the intercept and slope of the response function in the team-player treatment SEQ-TEAM. We do not have non-parametric tests for follower quantities directly as such comparisons leads to control for leader quantities. Last but not least, we find that group players produce less jointly and earn more than individual players in the squential treatments.

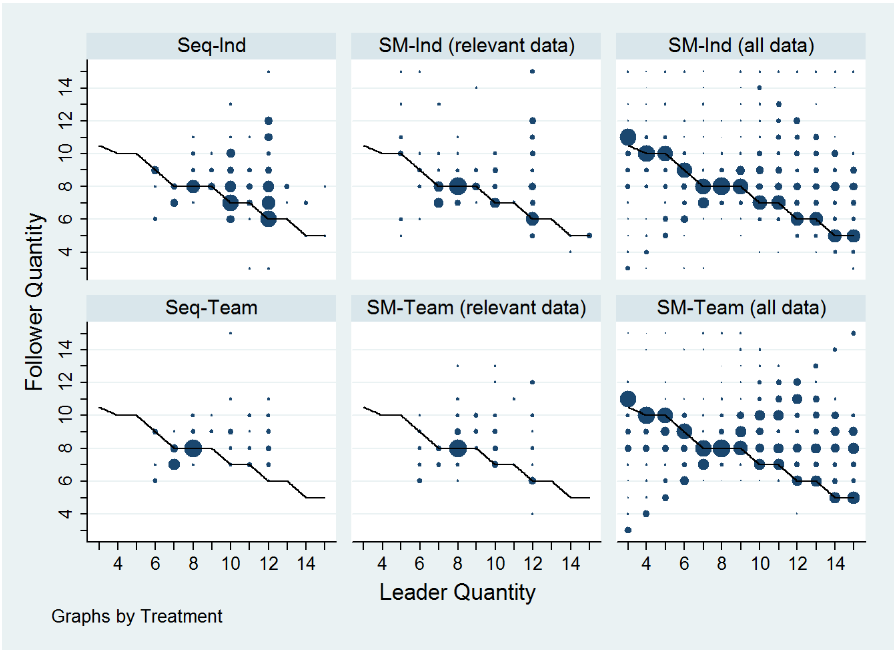
In contrast of the many differences found in the sequential treatments, average first-mover choices in treatment SM-IND and the corresponding group treatment SM-TEAM do not differ significantly. For completeness, in column 3 and 4 of Table 4.3 we also report results across the two individual and the two team treatments. These results show that the differences brought by the use of elicitation method are in general weak.

Table 4.3: Results of statistical tests for first- and second movers

Comparison based on player types		Comparison based on elicitation method	
SEQ-IND versus SEQ-TEAM	SM-IND versus SM-TEAM	SEQ-IND versus SM-IND	SEQ-TEAM versus SM-TEAM
Leader quantity — Parametric: estimates for the coefficient $\beta_1 . H_0 : \beta_1 = 0$. *Non-parametric: Mann-Whitney ranksum tests*			
2.132***	0.485	1.209***	-0.373
(0.399)	(0.607)	(0.470)	(0.302)
$p < 0.05$	$p = 0.394$	$p = 0.109$	$p = 0.468$
Follower quantity — Parametric: estimates for the coefficient $\beta_3 . H_0 : \beta_3 = 0$.			
0.324***	0.195**	0.254	0.013
(0.078)	(0.090)	(0.293)	(0.035)
Total quantity			
$p < 0.05$	$p = 0.670$	$p = 0.229$	$p = 0.564$
Total profit			
$p < 0.05$	$p = 0.670$	$p = 0.262$	$p = 0.387$

Notes: Estimated equation for leader quantities: $q^L_{ijt} = \beta_0 + \beta_1 \times TREATM + \varepsilon_{ijt}$, where q^L_{ijt} is the quantity chosen by first-mover subject/group i in session j in period t and $TREATM$ is a dummy used to code the treatments included in the regressions. In all regressions, the dummy variable $TREATM$ is coded such that it is equal to 1 for the treatment mentioned in the upper entry in each column of this table and it is equal to 0 for the treatment mentioned in the lower entry in each column. The coefficient β_1 measures the difference in average first-mover quantities in the two treatments included in the regression. A test of the hypothesis $H_0 : \beta_1 = 0$ will show whether or not the difference is significant. For testing behavior of the followers, we estimate the equation $q^F = \beta_0 + \beta_1 \times TREATM + \beta_2 q^L + \beta_3 \times TREATM \times q^L + \varepsilon_{ijk}$. In all regressions, the dummy variable $TREATM$ is coded such that it is equal to 1 for the treatment mentioned in the upper entry in each column of this table and it is equal to 0 for the treatment mentioned in the lower entry in each column. In order to account for possible non-independence of observations, we ran the regressions clustering data by subject or group and by session and using general linear latent and mixed models, GLLAMM (S.Rabe-Hesketh and A.Skrondal (2005). We report as p-levels $P > |t|$. *** indicates significance at the 1% level. Standard errors in parentheses. Using Tobit regression techniques delivers very similar results. None of the tests are significant for the 1-period treatments.

Figure 4.1: Distribution and frequency of choice pairs (Solid line is best response function)

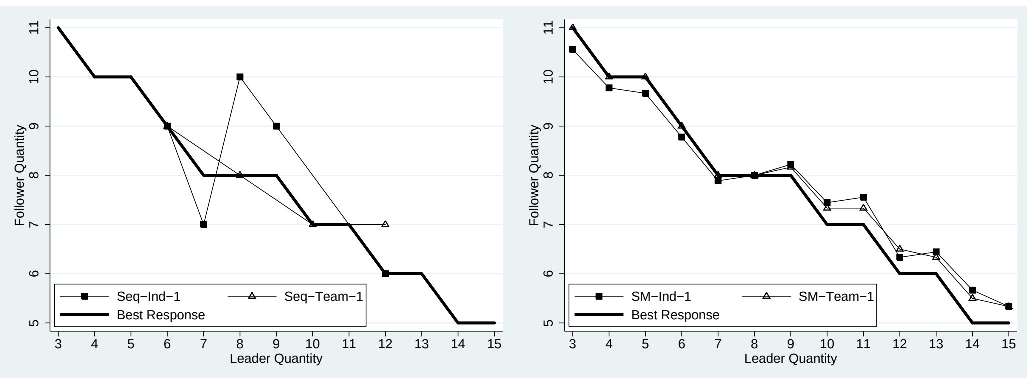


To learn about the distribution of first mover and, at the same time, about output pairs (q_L, q_F) of first and second mover choices, refer to Figure 4.1. Note that the larger is a dot in this figure, the more often the corresponding output pair was chosen. The solid line in the panels of Figure 4.1 represents the best-response function. Hence, this figure enables us to see how first and second mover choices match with each other, and how they deviate from the standard game theoretical prediction. Inspecting figure 4.1, we make a number of observations. First, in the group treatments, Cournot outcomes are most common (42.9% in SEQ-TEAM and 44.2% in SM-TEAM; 8.1% in SEQ-IND and 25.6% in SM-IND). In contrast, in the individual treatments there is more mass on outcomes that include first-mover quantities higher than the Cournot quantity of 8. This is particularly the case in treatment SEQ-IND where we observe a lot of pure Stackelberg outcomes (12, 6) (14.5% in SEQ-IND and 11.1% in SM-IND; 0.0% in SEQ-TEAM and 3.8% in SM-TEAM). Second, as already observed in Cox et al. (2007) in reference to the Huck et al. (2001) data, we see that the data in all panels of Figure 4.1 appear to be heteroscedastic as increasing (and decreasing, in case of all data of the strategy-method treatments) first mover choices lead to higher dispersion of second mover choices. Third, there is a lot of overproduction (which can be interpreted as punishment) by second movers for first mover quantities higher than 8, whereas we see underproduction (which can be interpreted as rewarding) for first mover quantities smaller than 8.

4.4.2 Second-mover behavior

Let us first consider second-mover behavior in the 1-period treatments. Figure 4.2 shows the average response function observed in the 1-period treatments (for the sequential-play treatments in the left and for the strategy-method treatments in the right panel). As the sequential-play treatments only deliver a few data points, no clear picture emerges in the left panel of Figure 4.2. If anything, the average response function of team players seems to be closer to the best response function than that of individual players in the sequential play treatments. A clearer picture emerges in the right panel showing the average response functions in the strategy-method treatments. We make two observations. First, for leader quantities smaller than the Cournot quantity of 8, the average team response function exactly coincides with the best-response function, whereas the average response function of individuals runs slightly below the best response function. The latter implies that individuals on average have a slight tendency to reward what could be interpreted as "nice" first-mover behavior. Second, for leader quantities larger than the Cournot quantity of 8, the average response functions of individual and teams are very similar and both run above the best response function, implying that both individuals and teams slightly punish what could be interpreted as "greedy" first-mover behavior. Although there is weak visual evidence indicating that the observed response functions of teams are closer to the best-response function than that of individual players in the 1-period treatments (which is in line with earlier results in the literature and our hypothesis), the estimation of simple response functions do not deliver any statistically significant differences.

Figure 4.2: Average response functions observed in the one-period sequential treatments (left) and the one-period strategy-method treatments (right)



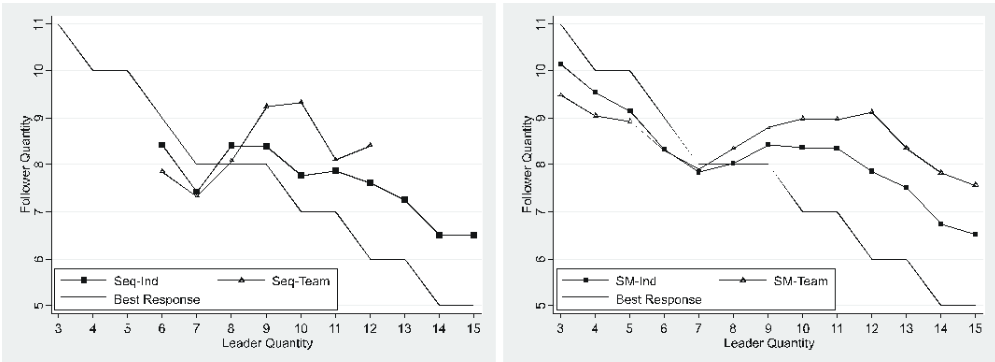
There are no observations for leader quantities 10 and 11 in treatment SEQ-IND-1.

Next turn to second-mover behavior in the 15-period treatments. The two panels in Figure 4.3 show the average response functions in the 15-period truly sequential (left panel) and the 15-period strategy-method treatments (right panel). Inspecting the two panels of Figure 4.3, it seems fair to state that the average observed response functions of team second movers are farther away from the best-response function than that of individual second movers in the 15-period treatments. Importantly, the two panels in Figure 4.3 as well as simple diagnostic tools: [16] This suggest that team second movers reward more and punish harder than individual followers. Interestingly, all observed response functions show a particular and perhaps somewhat surprising "first slope downward, then slope upward, then slope downward" pattern. This is most transparent in the strategy-method treatments. More precisely, the response functions in the strategy-method treatments are downward sloping for leader choices between 3 and 7, they slope upward for leader choices between 7 and 11/12, and then slope downward again for higher leader choices. Due to the more limited number of different choices of first movers in the sequential treatments, this pattern is less clear in the left panel of Figure 4.3. [17]

[16] Recall that the theoretical response function of followers is given by $q_F(q_L) = 12 - 0.5q_L$. Estimating such response functions as a quick diagnostic tool for our data and comparing the results of the relevant 15-period treatments delivers the following results (The details are provided in Web Appendix 4.A.) First, both the intercept and the slope of the response function employed in the individual-player treatment SEQ-IND-15 are significantly closer to the ones of the standard best-response function than the intercept and slope of the response function in the team-player treatment SEQ-TEAM-15. This suggests that individual second movers behave more "selfishly" than team second movers. Second, the reaction function in treatment Seq-Ind is downward-sloping, while the reaction function in treatment SEQ-TEAM is upward-sloping. This suggests that team followers reward more and punish harder than individual followers. Third, repeating this exercise for the "relevant" data (i.e., only second-movers' reactions at quantities actually chosen by first movers) in the 15-period strategy-method treatments confirms the result obtained for the truly sequential treatments.

[17] Reaction functions on the individual and group level show considerable heterogeneity ranging from best-response behavior to flat response functions (reflecting a basic reward-and-punishment scheme) to response functions that resemble the shape of those shown in the right panel of Figure 4.3. We come back to this issue in Section 4.5.

Figure 4.3: Average response functions observed in the 15-period sequential treatments (left) and the 15-period strategy-method treatments (right)



While estimated linear and monotonic response functions may serve as a quick diagnostic tools, from the preceding discussion we conclude that simple linear estimations are inappropriate and incapable of accounting for patterns observed in the average and individual response functions. Furthermore, although basic patterns are easily identifiable on the individual and team level in the strategy-method treatments, this is not easy in the sequential treatments as in the latter treatments we sometimes observe second-mover behavior only for a possibly small subset of first-mover quantities, which leads to identification and categorization issues. This raises two problems. First, how can we appropriately account for (average) response functions in the various treatments? Second, how can we formally compare second-mover behavior across treatments?

We can solve these problems by employing two recently suggested structural models. First of all, it turns out that the patterns observed in Figure 4.3 (and at the individual and group level) are consistent with the predictions of models of other-regarding preferences, especially the model by Fehr and Schmidt (1999). Therefore, in the next section we will account for followers' observed response functions by structural estimation of the Fehr and Schmidt's (1999) model of inequality aversion as suggested in Lau and Leung (2010). Furthermore, we also estimate and discuss Cox et al. (2007) model of emotion-driven reciprocity. For the time being we will ignore which other-regarding motive drives the results. The important point is that independent of the model we estimate, individuals appear to be more " self-regarding" than teams. We are able to make this statement as the standard selfish best response function is nested in both of these social preference models we estimate. Therefore, we have a clear and unambiguous method to decide which of two observed average response functions is closer to the prediction of subgame perfectness.[18]

[18] Surely, in the group treatments it is the group decision making process that maps individual mem-

4.4.3 A closer look at the data: structural estimations

Estimating a model of inequality aversion

Lau and Leung (2010) suggest that the experimental results of the Stackelberg markets reported in Huck et al. (2001) can be explained using a simplified version of the inequality-aversion model by Fehr and Schmidt (1999). In particular, Lau and Leung suggest that the population of second movers consists of a mixture of " standard" and " non-standard" preference types. Standard types are assumed to use the theoretical best-response function, whereas non-standard types are assumed to act as if maximizing a utility function of the Fehr and Schmidt type. In their paper, Lau and Leung first derive the response function of non-standard types. Interestingly, it turns out that this response functions accurately predicts the shape of the average response functions we observe in our data (see Figure 4.3). Lau and Leung then develop a maximum-likelihood model in which a share ϕ_{ns} of second movers are non-standard types and a share of $1 - \phi_{ns}$ of second movers are standard types. Estimating this model, using the random-matching Stackelberg data of Huck et al. (2001), they show that a substantial share (about 40%) of the second movers in Huck et al. (2001) appear to have preferences of the Fehr-Schmidt type. The fact that in our strategy-method treatment data we directly observe individual response functions that are consistent with either those of standard or non-standard types is a rationale to apply the model by Lau and Leung to our data to account for follower behavior. In the following we will shortly introduce the model put forward by Lau and Leung, closely following their exposition. We will then estimate it for our four treatments.

Denote player i and j's payoffs by π_i and π_j, respectively. Then, Fehr and Schmidt preferences are given by

$$u_i = \pi_i - \alpha_i \max\{\pi_j - \pi_i, 0\} - \beta_i \max\{\pi_i - \pi_j, 0\} \tag{4.1}$$

where $0 \leq \beta_i < 1$, $\beta_i \leq \alpha_i$, $i, j = L, F$ with $i \neq j$. The parameter α_i measures player i's aversion towards disadvantageous inequality, whereas the parameter β_i measures player i's aversion towards advantageous inequality. For estimation purposes, Lau and Leung make two assumptions. First, they assume that there are two types of second movers. The first type of second movers have standard selfish preferences and, hence, play according to the standard best response. These second movers are referred to as standard types (S). The second type of players have Fehr-Schmidt preferences and maximize utility as given in 4.1. These second movers are referred to as

ber's preferences into a decision of the group. Hence, in estimating these models also for the group treatments we maintain an as-if assumption, according to which a group's decision is a reflection of this "group's preferences." (See also Kocher and Sutter (2007), p.71) Given the specific non-monotonic shape of the observed response functions of groups and individuals, we employ these other-regarding preference models as a technical device in order to more adequately estimate and compare response functions.

non-standard types (NS). Second, Lau and Leung assume that all non-standard types have the same (dis)advantageous inequality parameter. Hence, $\alpha_i = a$ and $\beta_i = b$ for all non-standard players. Lau and Leung assume that the share of non-standard types in the population is given by $\phi_{ns} \in [0,1]$ where ϕ_{ns} is to be estimated from the data. Hence, the basic assumptions of Lau and Leung's simplified version of the Fehr-Schmidt model are as follows: $\Pr(\alpha_i = a \ \&\ \beta_i = b) = \phi_{ns}$, $\Pr(\alpha_i = \beta_i = 0) = 1 - \phi_{ns}$, where $0 \leq \phi_{ns} < 1, 0 \leq b < 1, b \leq a$.

Recall from above that a standard-type follower reacts according to the best response function given by $q_F^S(q_L) = 12 - \frac{1}{2}q_L$. Regarding the response function of non-standard followers, Lau and Leung show that it is given by

$$q_F^{NS}(q_L) = \begin{cases} 12 - \frac{q_L}{2(1-b)} & \text{if} \quad q_L \in A \\ q_L & \text{if} \quad q_L \in B \\ 12 - \frac{q_L}{2(1+a)} & \text{if} \quad q_L \in C \end{cases},$$

where $A = \left[3,\ 12\left(\frac{1-b}{3-2b}\right)\right]$, $B = \left[12\left(\frac{1-b}{3-2b}\right),\ 12\left(\frac{1+a}{3+2a}\right)\right]$, and $C = \left[12\left(\frac{1+a}{3+2a}\right),\ 15\right]$. Note that the best-response function is piecewise linear. [19] Note also that it slopes downward for low, slopes upward for intermediate, and slopes downward again for high first-mover quantities. Hence, it predicts the pattern observed in Figure 4.3. To briefly gain some intuition, consider the case of $q_L \in A$. Best responding to such a quantity choice, maximizes a second mover's profit but reduces utility of a non-standard type due to advantageous inequality. If q_L is small enough, the non-standard second mover finds it preferable to reduce quantity below the best response which reduces advantageous inequality by more than it decreases own profits.

To derive the likelihood function, let x_i and y_i represent the i th observed tuple of observed leader and follower choices. Lau and Leung assume that a follower with standard [non-standard] preferences chooses according $y_i = q_F^S(x_i) + \varepsilon_i$ $[y_i = q_F^{NS}(x_i) + \varepsilon_i]$, where ε_i is *iid* according to a normal distribution $N(0, \sigma^2)$ and $q_F^S(x_i)$ and $q_F^{NS}(x_i)$ are as given above. Since Lau and Leung assume a share ϕ_{ns} of non-standard and a share of $1 - \phi_{ns}$ standard second movers, the probability density of observing y_i is given by

$$(1 - \phi_{ns}) \times f_S(y_i|x_i;\sigma) + \phi_{ns} \times f_{NS}(y_i|x_i;a,b,\sigma)$$

where $f_S(y_i \mid x_i;\sigma)$ $[f_{NS}(y_i \mid x_i;a,b,\sigma)]$ is the probability density of observing y_i when the second mover has standard [non-standard] preferences.[20] The log likelihood function of observing the sample $(x_i, y_i)_{i=1}^{N_{\text{Treatm}}}$ of leader and follower choices is then given

[19] Note also that the standard best response is obtained when $a = b = 0$.

[20] Using the definition of the normal distribution, one obtains $f_S(y_i|x_i;\sigma) = \frac{1}{\sqrt{2\pi\sigma^2}} \exp\left(\frac{-\left(y_i - 12 + \frac{x_i}{2}\right)^2}{2\sigma^2}\right)$ and $f_{NS}(y_i) = f_A(y_i)^{1-D_B(x_i)-D_C(x_i)} \times f_B(y_i)^{D_B(x_i)} \times f_C(y_i)^{D_C(x_i)}$ where $f_A(y_i) = \frac{1}{\sqrt{2\pi\sigma^2}} \exp\left(\frac{-\left(y_i - 12 + \frac{x_i}{2(1-b)}\right)^2}{2\sigma^2}\right)$, $f_B(y_i) = \frac{1}{\sqrt{2\pi\sigma^2}} \exp\left(\frac{-(y_i - x_i)^2}{2\sigma^2}\right)$, and

Table 4.4: Estimation results for Lau-Leung's implementation of the Fehr and Schmidt model ($\phi_{ns} = 1$)

	Truly Sequential Play		Strategy Method			
			All Data		Relevant Data	
	SEQ-IND	SEQ-TEAM	SM-IND	SM-TEAM	SM-IND	SM-TEAM
ϕ_{ns}	1.000	1.000	1.000	1.000	1.000	1.000
	–	–	–	–	–	–
a	0.303***	0.629***	0.279***	0.641**	0.358**	0.500***
	(0.085)	(0.129)	(0.068)	(0.273)	(0.151)	(0.160)
b	0.216***	0.252***	0.192***	0.215**	0.215***	0.400***
	(0.029)	(0.017)	(0.062)	(0.090)	(0.035)	(0.001)
σ	1.506***	0.862***	1.485***	1.740***	1.658***	1.322***
	(0.164)	(0.190)	(0.200)	(0.423)	(0.263)	(0.224)
LL	-427.864	-198.222	-4599.221	-3334.489	-436.902	-262.242
N	234	156	2535	1690	227	156
Hypothesis Testing	$a^{\text{SEQ-IND}} = a^{\text{SEQ-TEAM}}$ & $b^{\text{SEQ-IND}} = b^{\text{SEQ-TEAM}}$		$a^{\text{SM-IND}} = a^{\text{SM-TEAM}}$ & $b^{\text{SM-IND}} = b^{\text{SM-TEAM}}$		$a^{\text{SM-IND}} = a^{\text{SM-TEAM}}$ & $b^{\text{SM-IND}} = b^{\text{SM-TEAM}}$	
	$p = 0.075(\chi^2_2 = 5.17)$		$p = 0.077(\chi^2_2 = 5.14)$		$p = 0.000(\chi^2_2 = 28.72)$	

by

$$\begin{aligned}&\ln L\left(a, b, \phi_{ns}, \sigma; (x_i, y_i)_{i=1}^{N_{\text{Treatm}}}\right)\\ = &\sum_{i=1}^{N_{\text{Treatm}}} \ln\left\{(1-\phi_{ns}) f_S(y_i) + \phi_{ns}\left[f_A(y_i)^{1-D_B(x_i)-D_C(x_i)} \times f_B(y_i)^{D_B(x_i)} \times f_C(y_i)^{D_C(x_i)}\right]\right\}\end{aligned}$$

where N_{Treatm} is the number of observations in the treatment under consideration. To control for non-independence of observations, we cluster standard errors on individuals or groups.

In an effort to first estimate the average response functions, as shown in Figure 4.3, we set $\phi_{ns} = 1$, that is, in a first step we assume that there are only non-standard types. The estimation results are given in Table 4.4.

We note that the parameter estimates of the inequality-aversion parameters a and b are significantly different from 0 in all treatments and data sets. Note also that the parameter estimates of a and b are in line with the restrictions $0 \leq b < 1$ and $b \leq a$ imposed by the Fehr and Schmidt model. Most importantly for the purpose of deciding which observed average response function is closer to standard best-response

$f_C(y_i) = \frac{1}{\sqrt{2\pi\sigma^2}} \exp\left(\frac{-\left(y_i - 12 + \frac{x_i}{2(1+a)}\right)^2}{2\sigma^2}\right)$, with the indicator variables defined as $D_B(x_i) = \begin{cases} 1 & \text{if} \quad 12\left(\frac{1-b}{3-2b}\right) < x_i \leq 12\left(\frac{1+a}{3+2a}\right) \\ 0 & \text{otherwise}\end{cases}$ and $D_C(x_i) = \begin{cases} 1 & \text{if} \quad 12\left(\frac{1+a}{3+2a}\right) < x_i \\ 0 & \text{otherwise}\end{cases}$.

Table 4.5: Estimation results for Lau-Leung's implementation of the Fehr and Schmidt model

	Truly Sequential Play		Strategy Method			
			All Data		Relevant Data	
	SEQ-IND	SEQ-TEAM	SM-IND	SM-TEAM	SM-IND	SM-TEAM
ϕ_{ns}	0.277	0.773***	0.276***	0.418***	0.279***	0.543***
	(0.175)	(0.077)	(0.011)	(0.015)	(0.092)	(0.132)
a	1.713	0.949***	1.035***	1.479***	15.584	3.327**
	(1.513)	(0.256)	(0.029)	(0.054)	(15.727)	(1.147)
b	0.383	0.470***	0.823***	0.828**	0.605***	0.476***
	(0.673)	(0.034)	(0.004)	(0.005)	(0.020)	(0.024)
σ	1.094***	0.717**	0.818***	1.034***	0.968***	0.931***
	(0.117)	(0.200)	(0.013)	(0.020)	(0.140)	(0.190)
LL	-426.523	-196.848	-4149.451	-3232.943	-377.509	-241.571
N	234	156	2535	1690	227	156
Hypothesis Testing	$a^{\text{SEQ-IND}} = a^{\text{SEQ-TEAM}}$ & $a^{\text{SEQ-IND}} = b^{\text{SEQ-TEAM}}$ $p < 0.001(\chi^2_2 = 40.93)$		$a^{\text{SM-IND}} = a^{\text{SM-TEAM}}$ & $b^{\text{SM-IND}} = b^{\text{SM-TEAM}}$ $p = 0.006(\chi^2_2 = 7.634)$		$a^{\text{SM-IND}} = a^{\text{SM-TEAM}}$ & $b^{\text{SM-IND}} = b^{\text{SM-TEAM}}$ $p = 0.002(\chi^2_2 = 9.82)$	

function (characterized by $a = b = 0$), we observe that both the disadvantageous inequality parameter a, and the advantageous inequality parameter b is larger in the team treatment than in the relevant individual treatment. For instance, while in SEQ-TEAM the parameter a is estimated as 0.629, is only 0.303 in treatment SEQ-IND. This is in contrast to the main hypothesis according to which the observed response function of teams should be closer to the standard best-response function than the one of individuals. The test results reported on the bottom of Table 4.4 indicate that we can (weakly) reject the hypothesis that in each of two relevant treatments comparisons the parameters a and b are the same.

We next estimate the full model, dropping the restriction $\phi_{ns} = 1$, and concentrate on the estimated share of standard and non-standard types in two related treatments. The results are shown in Table 4.5.[21] With the exception of treatment SEQ-IND, the share ϕ_{ns} of nonstandard types is estimated to be significantly larger than 0 in all treat-

[21] 10 out of 4998 choice pairs result in negative payoffs to both players (1 in SEQ-TEAM; 3 in SM-IND relevant data; 5 in SM-TEAM all data and 1 in SM-IND all data). Since the utility function in (4.2) is defined only for non-negative payoffs, we truncate these observations at $q_F = 24 - q_L$ which implies zero payoffs for both players. Furthermore, seven observations in treatment SM-IND, second movers reacted with quantities above the best-response to first-mover quantities smaller than 8. A possible explanation is that individual second movers exposed to the strategy method are likely to make more errors, especially at first mover quantities they do not actually observe very often in the course of the experiment. In the SM treatments (all data), observations from three individuals and two teams were dropped due to extreme responses to leader quantity 3 and 15, causing difficulties in finding convergence.

ments and range from about 0.27 in the individual treatments to 0.773 in treatment SEQ-TEAM. More importantly for our purposes, the share of non-standard types is estimated to be consistently higher in the group treatments than in the corresponding individual treatments. These differences are highly significant in all treatments (and data sets) as indicated by the test results presented at the bottom of Table 4.5.[22] This again is strong evidence against our main hypothesis according to which groups are expected to be more in line with the predictions of subgame perfectness.

Estimating a model of reciprocity

Recently, the behavior of second-movers in Stackelberg markets was also accounted for by a model of emotion-driven reciprocity (Cox et al. (2007)). Surely, next to or besides inequality aversion, reciprocity is a motivational force for second-mover behavior. Furthermore, the response function of the Cox-Friedman-Gjerstad model is flexible enough to, in principle, rationalize the shape of the observed average response functions shown in Figure 4.3. Therefore, as a robustness check of our finding that team second-movers are less (myopically) selfish than individual second-movers, we also estimated the model put forward by Cox, Friedman, and Gjerstad. We present the details in Web Appendix 4.A , but note here that the estimation results show that the " emotional state" of groups is more pronounced (both positively and negatively) than that of individuals. In particular, an estimated reciprocity parameter is significantly larger in the groups treatments than in the corresponding individual-player treatments. Hence, the results of this robustness exercise show that team followers appear to behave more reciprocal (or less self-regarding) than individual followers. This is, again, not in line with our main hypothesis.

4.5 Discussion

4.5.1 A potential explanation of the results

Summarizing our results derived so far, we can state the following. In the one-shot treatments we find weak evidence that is in line with previous results reported in the literature according to which groups are closer to the subgame perfect equilibrium prediction than individuals (although the differences we find are small and not significant). In our 15-period treatments, in contrast, we find that in comparison to individuals, groups choose lower quantities as first movers and reward more and punish harder as second movers. In other words, groups in our repeated game treatments appear to be less "selfish" than individuals. This raises the question of how the different results in our and the earlier experiments can be explained. We believe that

[22]We apply Wald test for testing parameter significance. We first accommodate data from different treatments into a large, unrestricted model. Then we put restrictions on coefficients to see whether they are equal to zero.

a possible explanation rests on the observation that there is substantial *heterogeneity in subjects' types* and the fact that different *time horizons* were used in our and the earlier experiments.

Regarding heterogeneity of subjects' types, we present substantial evidence that most subjects belong to one of three categories: (myopic) profit maximizer (PM), strategic rewarder and punisher (Strat-R&P), otherregarding preference-driven rewarder and punisher (Pref-R&P) (can be equality aversion/reciprocity). We will identify these types by concentrating on second-mover behavior, which is easily interpretable. PMs always maximize their payoff in response to any first-mover choice. S-R&Ps reward "nice" low leader quantities and punish "greedy" high leader quantities during all but the final round, where they revert to best response. These types arguably want to strategically " educate" leaders to choose lower quantities, until the final round where they revert to opportunistic behavior. Pref-R&P behave like Strat-R&Ps in all rounds. Since these types do not revert to payoff maximizing behavior even in the final round, their reward and punishment behavior can be interpreted as stemming from other-regarding preferences. Note that the existence of such or similar types has been reported in other studies in the literature (see, e.g., Luhan et al. (2001) and especially Reuben and Suetens (2009) for the existence of Strat-R&Ps and Pref-R&Ps).

Many earlier experiments reporting groups to be more " selfish" than individuals (see Section 2) employ one-shot interaction between subjects. In contrast, we have subjects interact repeatedly over 15 rounds (using random re-matching of individuals and teams). We believe that the heterogeneity in subjects' types and the different time horizons could explain the different results in our and the earlier experiments. For this purpose, let us first consider the case of one-shot interactions. Assume that subjects are either of the three types mentioned above. Of those, PMs and Strat-R&Ps will behave according to subgame perfect behavior while Pref-R&Ps will deviate from this behavior by displaying otherregarding concerns. Hence, behavior in inter-individual one-shot treatments is likely to be a mixture of selfish and other-regarding behavior. However, in the one-shot team treatments it is conceivable that both PMs and Strat-R&Ps convince the potentially present Pref-R&Ps that deviation from subgame-perfect behavior is not meaningful in a one-shot interaction. For instance, they might, given the first mover quantity, convince a group member who is an emotion-driven reciprocator to control feelings and to also vote for myopic best-response behavior. Hence, behavior in inter-group one-shot treatments is likely to be more homogeneous and more in line with the prediction of standard game theory. This would explain why in earlier experiments groups were on average found to be more selfish than individuals.

Consider now the case of multiple-period interactions. In the inter-individual treatments, average behavior will be a mixture of other-regarding behavior (displayed by both Pref-R&Ps and Strat-R&Ps) and PMs. However, in the multiple-round team treatments it is conceivable that Strat-R&P now side with Pref-R&Ps in an effort to convince the potentially present PMs that more cooperative behavior (established by reward and punishment) is the better thing to do (in the sense of achieving higher payoffs overall) when the game is repeated multiple times. Hence, behavior in inter-group multiple-round treatments is likely to be more homogenous and more in line with cooperative behavior. This would explain why in our experiment groups were on average found to be less selfish than individuals.[23] We believe that the mechanism we describe is are applicable to simultaneous-move dilemma games (such as prisoner's dilemma) and to sequential games that allow for competitive and cooperative outcomes (such as dictator, ultimatum, trust, or Stackelberg games). It is presumably less applicable to so-called eureka-type problems that have a " clear" solution that, ones discovered, is recognized as such (e.g., limit-pricing or beauty-contest game). In the remainder of this section, we provide evidence for the existence of the different types of subjects mentioned above.

4.5.2 Evidence for the explanation of the results

The first kind of evidence is provided by the estimation results of the Lau and Leung (2010) model presented in Section 4.4.3. There, the term $1 - \phi_{ns}$ measures the share of " standard" or best-response subjects. As this share is estimated to be significantly larger than 0, no matter which of the individual-treatment data sets we use, this provides evidence for the existence of myopic profit maximizers.

The second, more direct evidence is delivered by the inspection of the individual response functions in Figures 4.5 and 4.6 in Web Appendix 4.A. These Figures show the individual response functions of second movers in round 14 and 15, respectively, in treatment SM-IND. Inspecting the response function in Figures 4.5 and 4.6, we find the following categorization. *PMs:* Subjects 6, 9, 14, 16, and 18 are pure myopic profit maximizers. Moreover, subject 3 and 17 also play mostly best response, and could, hence, also be classified as myopic profit maximizer.[24] *Strat-R&P:* In period 14, subject 12 (13) basically plays best response for quantities smaller than 8 (9). In round 15, however, both subjects choose best response behavior for all first-mover

[23]Note that the mechanism we propose here where some subjects in a group try to convince other subjects of what is the " right" thing to do depending on the time horizon is in line with "Persuasive Argument Theory" (PAT) put forward in the psychological literature (see, e.g., J.A.F.Stoner (1961); Teger and Pruitt (1967)). PAT suggests that if the mean response of the individuals exhibits a preference towards a particular position, it is likely that the subjects will be exposed to more persuasive arguments in favor of this position during the discussion. Therefore, the ex-post group outcome will shift towards that particular initial position.

[24]Note that in treatment SM-Team, there are only 2 pure profit maximizers (teams 6 and 9) which can be seen in Figures 4.7 and 4.8 in Appendix 4.A. Hence, we observe a lower share of profit maximizers in the team treatment than in the individual treatment. This is consistent with our explanation above according to which, through team discussions, PMs are likely to be convinced to abandon their behavior in favor of some sort of reward-and-punishment behavior.

quantities. Hence, these two subjects can clearly be identified as strategic players. To a lesser extent, the same is true for subjects 10 and 11. The remaining subjects consist of those that can be classified as *Pref-R&P* and *"Others."* [25]

The third kind of evidence is provided by the analysis of follower chat protocols. We do this in view of illustrating two things: that statements made during the group discussions can be (albeit not exclusively) assigned to subject types mentioned above, and that many of the discussions can be easily characterized as a conflict between the types of subjects mentioned above. To economize on space, we only concentrate on followers in the main group treatment SEQ-TEAM-15 and SEQ-TEAM-1. Again, followers' discussions provide "richer" material.

We started the analysis by first listing all (interpretable) statements, proposals, motives, etc. that were voiced in any of the group chats. Then we tried to assign each of these statements to a broader category which would also reflect the type categories introduced above.[26] These categories were: PM, Strat-R&P, Pref-R&P, Non-PM, and " other." These categories are the column titles in Table 4.6. The complete list of all statements collected under the respective broad category is provided in the first column of Table 4.12 in the Web Appendix. Statements summarized in category Non-PM are those that, arguably, belong to either category Strat-R&P or Pref-R&P. However, an assignment to either of these categories is not unambiguous which is why we summarize them in a separate category.

The next step of the analysis was to try to briefly summarize each group's discussion in each round. It turned out that each discussion can be summarized by one of eight headlines, which provide the row titles in the upper part of Table 4.6. Here R stands for reward, PM for profit maximization, and P for punishment, respectively. The upper half of Table 4.6 is a cross table of the short summaries of chats' contents (column 1) and the broad categories of statements made during the chats (row 2). For instance, in the 23 cases that a round's chat could be summarized as "quick agreement on R," there was 1 statement attributable to a Strat-R&P motive, 5 statements attributable to a Pref-R&P motive, 30 statements attributable to Non-PM motive, and 6 statements that could not be summarized under a common headline.[27] A different cross table is provided in the lower half of Table 4.6. Here we cross the leader groups' quantity choices with the broad categories of statements made. (A more detailed overview of the cross table is provided in Tables 4.12 and 4.13 in the Web Appendix).

[25]These results are confimred by a hierarchical agglomerative cluster analysis (see Kaufman and Rousseeuw (1990)) of individual response functions. The details are available from the authors upon request.

[26]We attempt to identify the message itself instead of the subject who wrote the message. The latter is much more difficult due to the lack of information during communication, or changes of mind during the discussion, etc.

[27]Note that the sum of these statements do not sum up to 23, the number of observations listed in column 2 in Table 4.6. This is so because typically many different statements were made during one group's discussion in a round of the experiment.

Table 4.6: Analysis of chat protocols

		Categories of motives mentioned in group discussions				
Overall characterization of a round's discussion	# Obs.	PM	Strat R&P	Pref R&P	Non-PM	Other
Quick agreement on R	23		1	5	30	6
Quick agreement on PM	90	94		3	5	3
Quick agreement on P	15		2	13	13	7
PM vs R, R "wins"	10	18	19	9	12	2
PM vs R, PM "wins"	5	9	9	3	8	2
PM vs P, P "wins"	20	23	10	13	16	8
PM vs P, PM "wins"	7	20	1	4	6	5
How much P?	10	3	1	3	10	6
Σ	180	167 (41.5%)	43 (10.7%)	53 (13.2%)	100 (24.9%)	39 (9.7%)
Leaders' Choices						
$q_L = 6$	12	18	10	10	9	4
$q_L = 7$	42	31	13	6	34	5
$q_L = 8$	79	79	2	7	6	4
$q_L = 9$	4		2		5	3
$q_L = 10$	14	11	3	9	15	8
$q_L = 11$	9	11	4	5	8	5
$q_L = 12$	20	17	9	16	23	10
Σ	180	167	43	53	100	39

Note: Abbreviations used: R = Reward, PM = Profit maximization, P = Punishment.

The understandably less extensive categorization for treatment SEQ-TEAM-1 is provided in Table 4.7, which has a similar structure as 4.6.

With these preparations in place, we can come back to the two points we want to illustrate with the help of the chat protocols. First, we observe that also in the chat protocols we find ample evidence for various types of subjects. In fact, the column sums in the upper (or lower) part of Table 4.6 suggest that respectively 47%, 12.2%, and 15.0% of all interpretable statements made stem from subjects who can, respectively, be classified as (myopic) profit maximizers, strategic teachers, and otherregarding subjects. Second, row-wise inspection of Table 4.6 illustrates the conflicts that are carried out in group discussions. Surely, and almost tautologically, in cases in which there is quick agreement on an action, we typically only observe only one kind of argument. For instance, if there is quick agreement on best response (which typically happens in response to leader quantity 7 or 8, see the lower part of Table 4.6) there are almost no statements made in favor of a different action. On the other hand, if there is quick agreement on either reward or punishment, no statement is made in favor of

Table 4.7: Analysis of chat protocols in treatment SM-Team-1

Overall characterization of a round's discussion	No.obs.	Categories of motives mentioned in group discussions: PM	Non-PM	Other
PM vs R, PM "wins"	2	4	2	-
PM vs P, P "wins"	2	3	4	1
PM vs P, PM "wins"	2	6	2	5
$\sum$	6	13(48.1%)	8 (29.6%)	6 22.2(%)
Leaders' Choices				
$q_L = 6$	1	2	1	-
$q_L = 8$	2	5	2	2
$q_L = 10$	1	1	1	1
$q_L = 12$	2	5	4	3
$\sum$	6	13	8	6

Notes: Abbreviations used: R = Reward, PM = Profit maximization, P = Punishment. Percentages in row "$\sum$" refer to percentages of cases in the columns labled "Categories of motives mentioned in group discussions".

best response. The more interesting cases arise, of course, when a group's discussion can be characterized as a conflict between best response and a rewarding or a punitive action. In these cases we typically observe arguments and statements that can be attributed to all kinds of motives ranging from myopic profit maximization to strategic teaching to other-regarding and non-profit maximizing behavior. For instance, in the 10 group discussion that revolve around the question whether the leader group should be best responded to or be rewarded (and rewarding is the result), we observe 18 statements made in favor of profit maximization, and, respectively, 19, 9, and 12 statements in favor of strategic teaching, otherregarding motives, and non-profit maximization behavior. Not surprisingly, as there are many more statements made against best response, in these cases a response is chosen that rewards the leader's action. Similar patterns can be observed in the other discussions that are characterized by conflicts among group members. Note the fact that in conflict-laden group discussions it is typically the case that all kinds of arguments are exchanged, which can be seen by reading row-wise the lower part of Table 4.6. For instance in response to the collusive leader quantity $q_L = 6$, we see statements coming from all "camps." Likewise for higher leader quantities (≥ 10).

Table 4.8: Reactions of followers to previous leader quantities

	$q_L = 6$ (9)	$q_L = 7$ (14)	$q_L = 8$ (90)	$q_L = 9$ (6)	$q_L = 10$ (21)	$q_L = 11$ (4)	$q_L = 12$ (24)
Only leader q_L^{t-1}		7.14% (1)	3.33% (3)				8.33% (2)
q_L^{t-1} and neighborhood					4.76% (1)		4.17% (1)
q_L^{t-1} and other quantities not in the neighborhood	33.33% (3)	25.57% (4)	4.44% (4)	33% (2)	38.10% (8)	75% (3)	50% (12)
No change	66.67% (6)	64.29% (9)	86.67% (78)	67% (4)	52.38% (11)	25% (1)	27.5% (9)
Other quantities than q_L			5.56% (5)		4.76% (1)		

Notes: "Only leader q_L^{t-1}" means that a follower team only responds differently with respect to the leader quantity in the previous period. "q_L^{t-1} and neighborhood" means that a follower team changes response both regarding the leader quantity encountered in period $t-1$ and the leader quantity either to the left or to the right of q_L^{t-1}. "q_L^{t-1} and other quantities" means the response function changes not only at q_L^{t-1} and its neighborhood but also in other places. "Other quantities than q_L" means changes take place not at q_L^{t-1}. The numbers in the parentheses represents the absolute numbers of cases.

Table 4.8 reports the chat protocols of follower teams regarding the reactions to leader quantities. In the SEQ-TEAM treatments, followers simply react to the leader quantities they are paired up with, and hence there is no strategic uncertainty involved. In the following, we only include analysis for the SM-TEAM treatment.

Table 4.8 shows that for smaller leader quantities (especially for the Cournot quantity $q_L = 8$), the majority of team followers decide not to change their response functions. The main reason, through their chats, is that they believe the future leader quantities they match will remain the same and hence they are happy with the current strategies. Nevertheless, for larger leader quantities around the Stackelberg outcome, follower teams tend to change their strategies, not only with respect to the leader quantities they encountered, but also to other leader quantities along the reaction function. The reason could be that they are likely to be matched with a different leader in the subsequent period (probability 0.67), and hence it pays off to reconsider reactions to other possible occasions.

4.6 Summary and concluding remarks

In this study we compare the behavior of individuals and groups in a sequential market game in both one-period and multiple-period game treatments. Our main finding is a differential effect the time horizon of interaction has on the extent of individual and group players' (non)conformity with subgame perfectness. In the one-shot treatments we find that although on average groups appear to be somewhat closer to subgame perfectness than individuals, none of the differences in behavior are statistically significant. However, in the repeated game treatments we find that groups are less (myopically) selfish and more cooperative than individuals. These findings are to a large extent independent of the mode in which we elicit choices or the model we employ to account for second-mover behavior. Importantly, our main finding is in (stark) contrast to results in earlier studies reporting that groups appear to be more selfish than individuals. A possible explanation for the different results in our and earlier studies is that there is heterogeneity in subjects' types, ranging from pure (myopic) profit maximization to either strategic or preference-driven reward-and-punishment behavior. Depending on the time horizon of the interaction, the exchange of persuasive arguments via discussions is likely to lead groups to (possibly) more selfish behavior in one-shot interactions and to more cooperative behavior in repeated interactions. Since subjects in inter-individual interactions can not exchange arguments regarding what constitutes "meaningful" behavior in the face of different features of the interaction, it is conceivable that their behavior reacts to a lesser extent to the time horizon of interaction. Our main result implies that the statement "Groups, it seems, are more selfish and more sophisticated players than individuals, and, as a result, interactions between two unitary groups are closer to the standard, game-theoretical solution than interactions between two individuals." (Bornstein 2008, p. 30), which summarizes much of the previous literature on interindividual and intergroup comparisons in simple, sequential-move games, needs modification.

Our results show that the second part of the above statement does not generally apply to multiple-period game settings. In fact, for games that leave relatively more room for otherregarding preferences, the time horizon of interaction seems important, leading the play of groups either closer or farther away from the game-theoretic prediction than that of individuals. In the light of our results (and to the extent that the explanation of our results is convincing) it might be worthwhile to revisit other simple sequential-move games (such as the ultimatum game, the trust game, the centipede game, and the gift-exchange game) to check for a possible differential effect of the time horizon of interaction. While we concentrate on the effect of the time horizon of interaction in interindividual and intergroup comparisons, much more research is called for to analyze the effect of other design features such as the nature of communication within groups (e.g., face-to-face or anonymous chat) or the voting mechanism (e.g., majority or unanimity voting). [28]

The Stackelberg market game is, arguably, not of the "Eureka" type, where a solution once found is recognized as such by players. Therefore the results of our re-

[28] Some studies, such as Elbittar et al. (2004), Gillet et al. (2009b), Gillet et al. (2009a) vary the nature of managerial decision making processes within firms and analyse their impact on intergroup and interindividual firm behavior.

peated markets are not necessarily in contrast to the findings summarized by the second quote in the Introduction, which summarizes results from repeated interaction in games with a strong "Eureka" component. In these games, behavior of groups was shown to converge much faster to the (same) game-theoretic prediction than individuals. However, our repeated-game results show that neither groups nor individuals converge to a (refined) game-theoretic prediction, and, what is more, that groups clearly diverge farther from it than individuals (see also Cox and Hayne (2006) and Sutter et al. (2009)).

It is one question to check who is closer to game-theoretic predictions in interindividual and intergroup comparisons, another question is to check who earns higher profits. In particular and perhaps not surprisingly, there does not seem to be a simple relationship between higher conformity with game-theoretic predictions and higher profits. For instance, Feri et al. (2010) show that groups are significantly better at coordinating on more efficient outcomes and hence earn higher profits than individuals, while Bornstein et al. (2004) show that groups exit earlier in one-shot centipede games, leading to lower profits in comparison to individuals. On the other hand, Cox and Hayne (2006) and Sutter et al. (2009) show that in some auction formats, groups pay higher prices than individuals and are more often victim of the winner's curse than individuals and hence, groups make smaller profits than individuals. In our repeated Stackelberg markets employing truly sequential play, however, we find that groups earn significantly higher total profits than individuals, although groups' behavior is farther away from the (refined) game theoretic prediction. These results seem to suggest that more research is needed to explore when (type of game, etc.) and why (design features, ease of collusion, etc.) groups earn more than individuals. The answer to this question is important for a recommendation on when to entrust decision making to groups instead of to individuals in real-world settings.

Our results also speak to the extensive psychological literature on individual-versus-group decision making, especially regarding the so-called "discontinuity effect." This effect which, importantly, so far largely rests on observations in one-shot prisoner's dilemma games, refers to the finding of "intergroup interactions to be more competitive, or less cooperative, than interindividual relations" (Wildschut and Insko (2007), p. 175, emphasis added). Clearly, the results of our 15-period treatments show that, indeed, there is a clear difference or discontinuity between inter-individual and intergroup interaction. However, our results show that the "discontinuity" goes in the opposite direction than stated so far in the psychology literature. Hence, the definition of the discontinuity effect might need modification, too, accommodating, among other things, the time horizon of interaction. [29]

In this paper, we also make progress in terms of methodology regarding the com-

[29] Note that Lodewijkx et al. (2006) discuss the possibility of the time horizon to have a differential effect on interindividual versus intergroup comparisons. However, they do not provide convincing evidence for this claim.

parison of interindividual and intergroup behavior. First, we study both one-shot and multiple-period treatments in a unified framework, whereas other studies either only implement one-period or only multiple-period games. Second, in an additional set of treatments we employ the strategy-method to control for the possibility that differences in second-mover behavior observed across interindividual and intergroup treatments are driven by different experiences second movers make in the two environments. This also enables us to uncover the complete shape of the response function used by experienced Stackelberg followers. Independent of whether they were elicited from individual or group followers, average response functions in repeated Stackelberg markets display the same characteristic pattern. They slope downward for lower leader quantities, slope upward around the Cournot quantity and slope downward again for larger leader quantities. These results imply that it does not seem to be warranted to just run linear regressions to estimate followers' response functions in repeated games, as done, for instance in, Huck et al. (2001). [30] Interestingly, the specific shape of followers' response functions is nicely predicted by models of other-regarding preferences, such as Fehr and Schmidt (1999). Building on earlier contributions by Lau and Leung (2010) and Cox et al. (2007), we demonstrate that experienced followers' response functions are more adequately accounted for by estimating structural models of other-regarding preferences rather than by simple linear regressions. This allows us to unambiguously test which of two response functions is closer to the best-reply function, which can be view as a third methodological contribution of our paper.

[30] It remains to be checked whether similar unexpected patterns can be observed in other sequential-move games, such as price leadership or models of endogenous timing in oligopoly markets.

4.A Appendix

Design details of related studies

Table 4.9: Design details of related studies

Study	Game nication	Time horizon	Commu- size	Group rule	Voting
Cason and Mui (1997)	dictator	one-shot	face to face	2	unanimity
Luhan et al. (2009)	dictator	one-shot	electr. chat	3	unanimity
Bornstein and Yaniv (1998)	ultimatum	one-shot	face to face	3	unanimity
Robert and Carnevale (2002)	ultimatum	one-shot	face to face	2/4	unanimity
Cox (2002)	trust	one-shot	face to face	3	unanimity
Kugler et al. (2007)	trust	one-shot	face to face	3	unanimity
Kocher and Sutter (2007)	gift exchange	one-shot	face to face/electr. chat	3	unanimity
Bosman et al (2006)	power-to-take	one-shot	face-to-face	3	unanimity
Bornstein et al (2004)	centipede	one-shot	face to face	3	unanimity

Note: This table lists studies analysing intergroup versus interindividual behavior in sequential two-player games using one-shot interaction.

Results of simple linear response function estimations

As a quick diagnostic tool, we estimate simple linear response functions employed by second movers in the various treatments. We start with the truly sequential treatments and estimate the equation $q^F_{ijk} = \beta_0 + \beta_1 \times D_{\text{SEQ-TEAM}} + \beta_2 q^L + \beta_3 \times D_{\text{SEQ-TEAM}} \times q^L + \varepsilon_{ijk}$. In this equation, q^F_{ijt} is the quantity chosen by second-mover subject/group i in session j in period t. $D_{\text{SEQ-TEAM}}$ is a dummy that equals 1 if an observation stems from treatment SEQ-TEAM and equals 0 if an observation stems from treatment SEQ-IND.[31] The estimations results are as follows. (Recall that the standard best response function is given by $q^F = 12 - 0.5q^L$.)

$$
\begin{array}{llllll}
q^F = & \beta_0 & + \beta_1 \times D_{\text{SEQ-TEAM}} + & \beta_2 q^L & + \beta_3 \times D_{\text{SEQ-TEAM}} \times q^L + \varepsilon_{ijk} \\
 & 8.920^{***} & -2.570^{***} & -0.111^{***} & 0.324^{***} \\
 & (0.459) & (0.772) & (0.042) & (0.078)
\end{array}
$$

From these estimation results it follows that the observed response function in treatment SEQ-IND (when $D_{\text{SEQ-TEAM}} = 0$) is $q^F = 8.920 - 0.111q^L$ whereas the reaction function in treatment SEQ-TEAM (when $D_{\text{SEQ-TEAM}} = 1$) is $q^F = (8.920 - 2.570) +$

[31] As before, we cluster by subject and session to control for non-independence of observations and estimate the equation using general linear latent and mixed models, GLLAMM (Sophia Rabe-Hesketh and Anders Skrondal, 2005)

$(-0.111+0.324)q^L = 6.35+0.213\,q^L$. Hence, the reaction function in treatment SEQ-IND is *downward*-sloping, while the reaction function in treatment SEQ-TEAM is *upward*-sloping. This means that team followers reward more and punish harder than individual followers. Note that the estimates of the coefficients β_1 and β_3 are statistically significantly different from 0. This indicates that both the intercept and the slope of the response function employed in the individual-player treatment SEQ-IND are significantly closer to the ones of the rational best-response function than the intercept and slope of the response function in the team-player treatment SEQ-TEAM. Hence, it unambiguously appears that individual second movers behave more "rationally" than team second movers. Again, this contradicts our main hypothesis. Given the more "rational" behavior of individuals, it is no surprise that we observe individual first movers in the truly sequential treatment to choose on average higher quantities than team first movers (see Table 4.3).

Surely, the result of more reciprocal behavior in the sequential team treatment compared to the sequential individual treatment might be due to the different experience second movers make in the two treatments. To control for this feature, we estimate a similar equation as above using the data observed in the strategy-method treatments. For this purpose, we include all data of the complete response functions elicited in the strategy-method treatments. Hence, we compare individual and team second-movers on more equal grounds as we take their reaction at all first-mover quantities into account. The estimation results are as follows.

$$\begin{array}{llllll} q^F = & \beta_0 & +\,\beta_1 \times D_{\text{SM-TEAM}} + & \beta_2 q^L & +\,\beta_3 \times D_{\text{SM-TEAM}} \times q^L + \varepsilon_{ijk} \\ & 10.354^{***} & -0.955^{***} & -0.233^{***} & 0.152^{***} \\ & (0.087) & (0.142) & (0.009) & (0.014) \end{array}$$

From these estimation results we infer that the observed response function in treatment SM-IND is $q^F = 10.354 - 0.233q^L$ whereas the one in treatment SM-TEAM is $q^F = 9.399 - 0.081\,q^L$. Hence, we find that the slope is negative in both treatments. However, as in the truly sequential treatment we find that the intercept (slope) of the observed response function in the individual treatment is significantly larger (smaller) than the intercept (slope) of the observed response function in the team treatment (see the significance levels of the estimated coefficients β_1 and β_3). This, again, implies that individuals appear to be more "rational" than teams.

Note finally that once we consider only " relevant" data in the strategy-method treatments (i.e., only second-movers' reactions at quantities actually chosen by first movers), we again find that individuals' reaction function is (slightly) downward sloping (SM-IND: $q^F = 8.543 - 0.089\,q^L$) while teams' reaction function is upward sloping ($q^F = 7.615 + 0.106\,q^L$). This can be inferred from the following estimation results:

$$q^F = \underset{\substack{8.543^{***} \\ (0.415)}}{\beta_0} + \beta_1 \times \underset{\substack{-0.928 \\ (0.843)}}{D_{\text{SM-TEAM}}} + \underset{\substack{-0.089^{**} \\ (0.044)}}{\beta_2 q^L} + \beta_3 \times \underset{\substack{0.195^{**} \\ (0.090)}}{D_{\text{SM-TEAM}} \times q^L} + \varepsilon_{ijk}$$

As before, we find that the reaction functions of individual second-movers appear to be closer to the rational best-response than reaction functions of group second movers.[32]

Estimating a model of reciprocity

In this section we provide the details of the estimation of the emotion-driven reciprocity model of Cox, Friedman, and Gjerstad (2007) briefly mentioned in Section 4.4.3. In doing so, we closely follow their exposition. Cox-Friedman-Gjerstad postulate that agents have preferences over own or " my" (m) and other or " your" (y) payoffs that are represented by the following utility function:

$$u(m,y) = \begin{cases} (m^\alpha + \theta y^\alpha)/\alpha & \text{for} \quad \alpha \in (-\infty, 0) \cup (0,1] \\ (my^\alpha) & \text{for} \quad \alpha = 0. \end{cases} \tag{4.2}$$

The " convexity" parameter α determines the shape of the indifference curves. For $\alpha = 1$ preferences are straight lines, whereas they are strictly convex for $\alpha < 1$. The parameter θ represents the emotional state of an agent and is a function of the reciprocity and status variables r and s. Since player positions are randomly assigned to subjects rather than earned, Cox-Friedman-Gjerstad suggest to assume $s = 0$ in our case. The reciprocity variable r is a function $r = r(x) = m(x) - m_0$ where $m(x)$ is the maximum payoff the second mover can guarantee himself after the first mover's choice of x, and $m_0 = m(x_0)$ is the second mover's payoff for a " neutral" choice x_0 by the first mover, which will be estimated from the data. Cox-Friedman-Gjerstad suggest to normalize $r(x)$ such that it lies in the interval $[-1, 1]$. With let $m_g = \max_x m(x)$ and $m_b = \min_x m(x)$, the normalized reciprocity is given by $r(x) = (m(x) - m_0)/(m_g - m_b)$, when $m_g > m_b$, and $r = 0$ otherwise. Given the first mover's choice $x \in \{3, 4, ..., 15\}$ in our game, we obtain $m(x) = (12 - 0.5x)^2$, and $m_g - m_b = m(3) - m(15) = 90$. Hence, $r(x) = \left((12 - 0.5x)^2 - (12 - 0.5x_0)^2\right)/90$ for a proper first-mover choice x_0. For estimation purposes, Cox-Friedman-Gjerstad impose two assumptions. Assumption A.1: Agents make choices to maximize the utility function given in equation (4.2). Assumption A.2: The emotional state function $\theta = \theta(r)$ is the same for all agents except for a mean zero idiosyncratic term ε. Hence, $\theta_i = \theta(r) + \varepsilon_i$.

Instead of assuming a specific distribution, Cox-Friedman-Gjerstad suggest to have the data select the error distribution. We use the following error power exponential

[32] Using Tobit regression techniques delivers very similar results.

distribution with density

$$f(z;\mu,\sigma,\nu) = \frac{\nu \exp(-0.5\,|z/d\sigma|^{\nu})}{2^{1+1/\nu}\Gamma(1/\nu)\sigma d},$$

where $d = \left(2^{-2/\nu}\Gamma(1/\nu)/\Gamma(3/\nu)\right)^{0.5}$ for $z \in (-\infty,+\infty)$, $\mu \in (-\infty,+\infty)$, $\sigma,\nu > 0$. The parameters σ and ν will be estimated from the data. [33]

Cox-Friedman-Gjerstad show that the emotional state of a subject can be written as $\theta_i = ar(x) + \varepsilon_i$
$= a\left((12-0.5x)^2 - (12-0.5x_0)^2\right)/90 + \varepsilon_i$, where a is a reciprocity parameter which, again, will be estimated from the data. Note that the emotional state of a follower reacts to the difference of the maximal payoff given a leader's choice x and the maximal payoff for the neutral leader choice x_0. With the above definitions in place, write the utility function (4.2) in terms of players' choices. That is, substitute the payoff functions $m(x,q) = (24-x-q)q$ and $y(x,q) = (24-x-q)x$ to get

$$u_i(x,q) = \begin{cases} (24-q_L-q_F)^{\alpha}(q_F^{\alpha}+\theta_i q_L^{\alpha})/\alpha & \text{for} \quad \alpha \in (-\infty,0)\cup(0,1] \\ (24-q_L-q_F)^{1+\theta_i}(q_F q_L^{\theta_i}) & \text{for} \quad \alpha = 0. \end{cases} \tag{4.3}$$

The first-order condition of (4.3) w.r.t. q is $(24-x-2q)q^{\alpha-1} - \theta_i x^{\alpha} = 0$.[34] Cox-Friedman-Gjerstad show that this FOC is valid for all $\alpha \le 1$ and that a unique maximizer $q^*(x,\alpha,\theta) = q^*(x,\alpha,a,x_0)$ of function (4.3) exists for all $(\theta,\alpha) \in (-\infty,\infty)\times(-\infty,1]$. Summarizing, the goal of the estimation is to find α (the convexity parameter), a (the reciprocity parameter), x_0 (the reference choice of the first mover) and b and c (the parameters of the error distribution) by maximizing the log likelihood function

$$\ln L(\alpha,a,x_0,\sigma,\mu;x,q) = \prod_{i=1}^{N_{\text{Treatm}}} \ln \Pr\left[q_i = q | x_i,\alpha,a,x_0,\sigma,\mu\right]$$

for the N_{Treatm} observations in the treatment under consideration. To control for non-independence of observations, the model was estimated with robust standard errors and with observations clustered by individual subject or group.

The estimation and test results are displayed in Table 4.10.[35] The parameter α (ranging from negative infinity to 1) measures the slope of the utility function. The parameter a captures the sensitivity of a follower's reciprocity: The larger the a, the more strongly a follower reacts to the changes of a leader quantity. The parameter x_0 represents the leader quantity that triggers best response from a follower. We make the following observations.

[33]The probability density of the exponential power error function used in CFG is: $f(z;b,c) = \frac{\exp\left(-0.5|z/b|^{2/c}\right)}{b2^{c/2+1}\Gamma(c/2+1)}$. There is one to one correspondence between our parameters and theirs ($b = d\sigma$, $c = 2/\nu$).

[34]Note that the standard best response is obtained for $\alpha = 1$, $a = 0$, and $x_0 = 12$.

[35]The same notes as for the estimation of the Fehr and Schmidt (1999) model apply (see footnote 21).

Table 4.10: Estimation results for the Cox-Friedman-Gjerstad model

	Huck et al (2001) data	Truly Sequential Play		Strategy Method			
				All Data		Relevant Data	
		SEQ-IND	SEQ-TEAM	SM-IND	SM-TEAM	SM-IND	SM-TEAM
α	0.285**	0.308**	-1.127***	0.901**	0.929***	0.4000	-0.207
	(0.129)	(0.151)	(0.075)	(0.050)	(0.040)	(0.247)	(0.269)
a	0.789***	0.977***	2.805***	0.131***	0.742***	0.406***	2.218***
	(0.101)	(0.144)	(0.070)	(0.010)	(0.026)	(0.094)	(0.514)
x_0	5.669***	8.108***	8.022***	7.013****	6.396***	8.677***	7.970***
	(0.564)	(0.336)	(0.046)	(0.067)	(0.124)	(0.359)	(0.109)
σ	0.494***	0.297***	0.170***	0.536***	0.560***	0.444***	0.297***
	(0.044)	(0.019)	(0.028)	(0.030)	(0.020)	(0.088)	(0.430)
ν	0.549***	1.400***	0.563***	0.395***	0.639***	0.413***	0.618***
	(0.116)	(0.199)	(0.145)	(0.019)	(0.026)	(0.080)	(0.116)
LL	–	-403.882	-161.284	-4273.041	-4144.851	-362.734	-232.151
N	220	234	156	2535	1690	227	156
Hypothesis Testing		$\alpha^{\text{SEQ-IND}} = \alpha^{\text{SEQ-TEAM}}$		$\alpha^{\text{SM-IND}} = \alpha^{\text{SM-TEAM}}$		$\alpha^{\text{SM-IND}} = \alpha^{\text{SM-TEAM}}$	
		$p < 0.001 (t = 8.511)$		$p = 0.662 (t = -0.437)$		$p = 0.097 (t = 1.662)$	
		$a^{\text{SEQ-IND}} = a^{\text{SEQ-TEAM}}$		$a^{\text{SM-IND}} = a^{\text{SM-TEAM}}$		$a^{\text{SM-IND}} = a^{\text{SM-TEAM}}$	
		$p < 0.001 (t = -12.679)$		$p < 0.001 (t = -21.934)$		$p < 0.001 (t = 3.468)$	
		$x_0^{\text{SEQ-IND}} = x_0^{\text{SEQ-TEAM}}$		$x_0^{\text{SM-IND}} = x_0^{\text{SM-TEAM}}$		$x_0^{\text{SM-IND}} = x_0^{\text{SM-TEAM}}$	
		$p = 0.799 (t = 0.254)$		$p < 0.001 (t = 4.378)$		$p = 0.060 (t = 1.884)$	

Notes: The estimations of Cox et al. (2007) using Huck et al. (2001) data are presented in the first column. The log-likelihood information is absent in the paper.

Our estimation results in the SEQ-IND treatment are similar to the ones using Huck et al. (2001) data except for "neural leader quantity" x_0. Individual followers in our dataset consider Cournot quantity 8 to be fair, while followers in the Huck et al. (2001) think the "fair" leader quantity is the collusion quantity 6.

Second, as the estimated reciprocity parameter a is significantly larger than 0, the emotional state θ is a positive function of the reciprocity r parameter in all treatments. More importantly for our main hypothesis, we find that the estimated reciprocity parameter a is larger in the group treatments than in the corresponding individual-player treatments. The test results shown at the bottom of Table 4.10 indicate that these differences are highly statistically significant. Hence, team followers appear to behave more reciprocal (or less " self-regarding") than individual followers. This is not in line with our main hypothesis.

Third, regarding the convexity parameter α , we find that its estimate in the group player treatment SEQ-TEAM is significantly lower than its estimate in the corresponding individual treatment SEQ-IND.[36] This means that, c.p., indifference curves of teams are shallower than those of individuals and, hence, teams are willing to give up more money in order to increase the leader's income by a unit than individuals (see Cox, Friedman, and Sadiraj, 2008). This is not in line with our main hypothesis. We obtain a similar result when only the relevant data in the strategy-method treatments are included in the estimation. Furthermore, when all data is of the strategy-method treatments are taken into account, the estimates of the convexity parameter α are pretty similar and much closer to 1 (when indifference curves are linear), and not significantly different from each other.

Fourth, the neutral first-mover output x_0 is close to the Cournot quantity of 8 in both of the truly sequential treatments. The estimates of the neutral first-mover quantity x_0 are much lower in the strategy-method treatments when all data are taken into account. We find $\widehat{x}_0 = 7.013$ for treatment SM-IND and $\widehat{x}_0 = 6.396$ for treatment SM-TEAM. The test result indicate that these two parameter estimates are statistically significantly different. Hence, team second-movers start punishing " earlier" (that is, for lower first-mover quantities) than individual second-movers. Note that the estimates of x_0 in the strategy-method treatments are clearly higher (and again closer to the Cournot quantity of 8) when only the relevant data is taken into account. The test result indicates that this difference is statistically significant. [37]

[36] Again, we use the Wald test to compare parameters, following similar procedures in the previous subsection. Since there is only one restriction on parameters, and we assume the parameter differences to be normal, it is statistically identical to a t-test.

[37] For the sake of comparison, CFG use the random-matching Stackelberg data of Huck, Müller, and Normann (2001) (which is closest to our treatment SEQ-IND) to estimate the same model. They find $\alpha = 0.285$, $a = 0.789$, and $x_0 = 5.669$.

Figure 4.4: Plot of the estimated emotional state variable θ

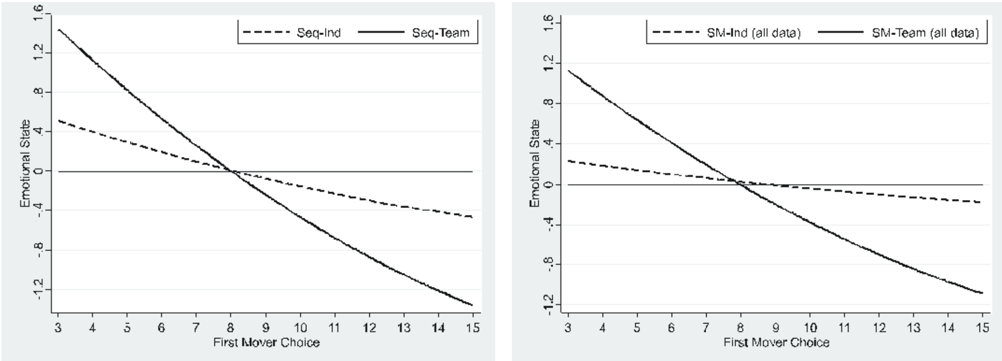


Figure 4.4 shows the plots of the emotional state function θ for our four treatments given the estimated values of the reciprocity parameter a and the neutral first-mover choice x_0. We note that both for the sequential as well as for the strategy-method treatments, the emotional state of groups is more pronounced (both positively and negatively) for groups than that of individuals. Furthermore, perhaps not surprisingly, the emotional state of both individuals and groups seems to react somewhat stronger to the (hot) sequential treatment compared to the (cold) strategy-method treatments.

Complete response functions in SM treatments

Figure 4.5: Treatment SM-IND: Response functions in round 14

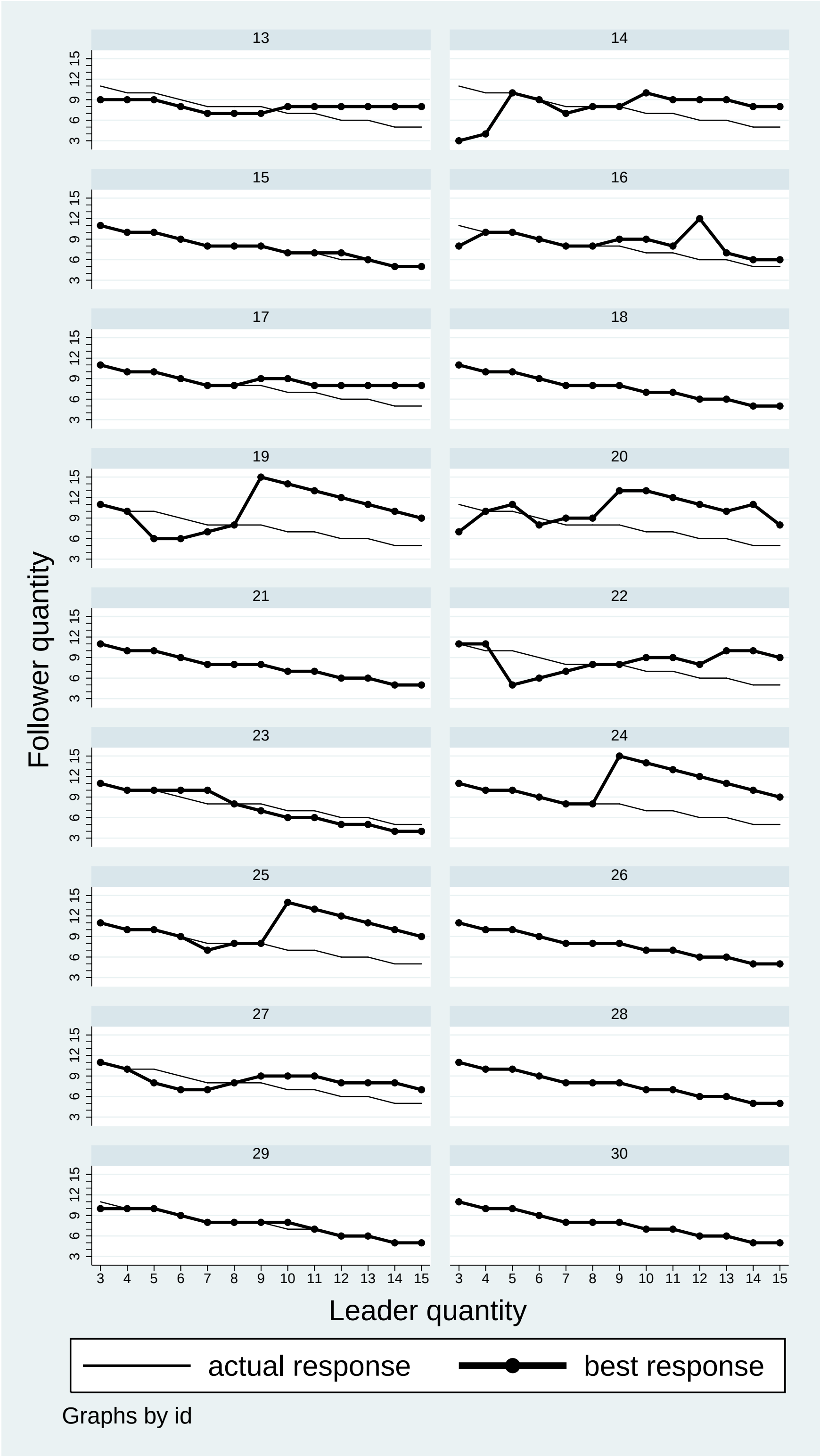


Figure 4.6: Treatment SM-IND: Response functions in round 15

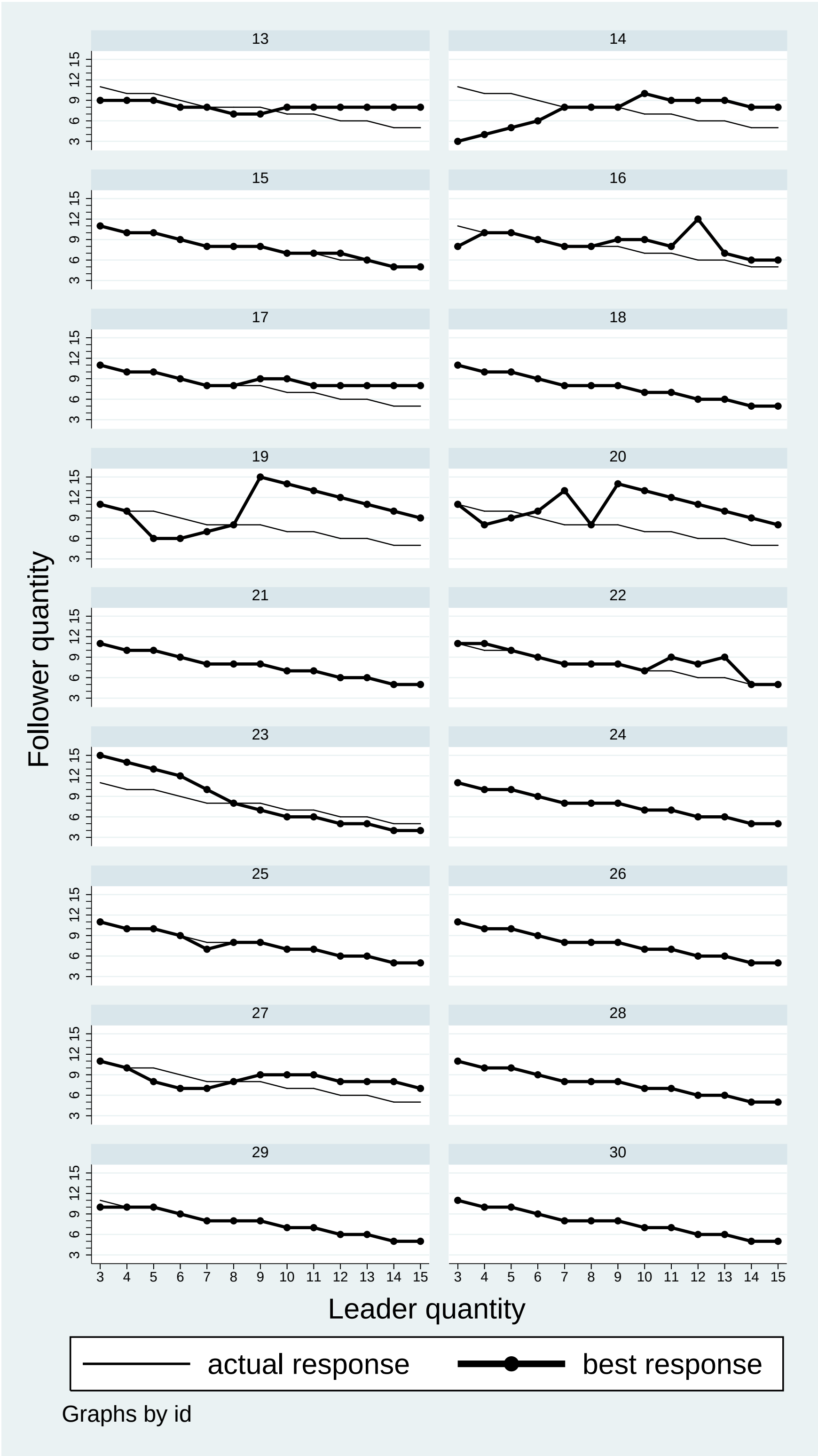


Figure 4.7: Treatment SM-TEAM: Response functions in round 14

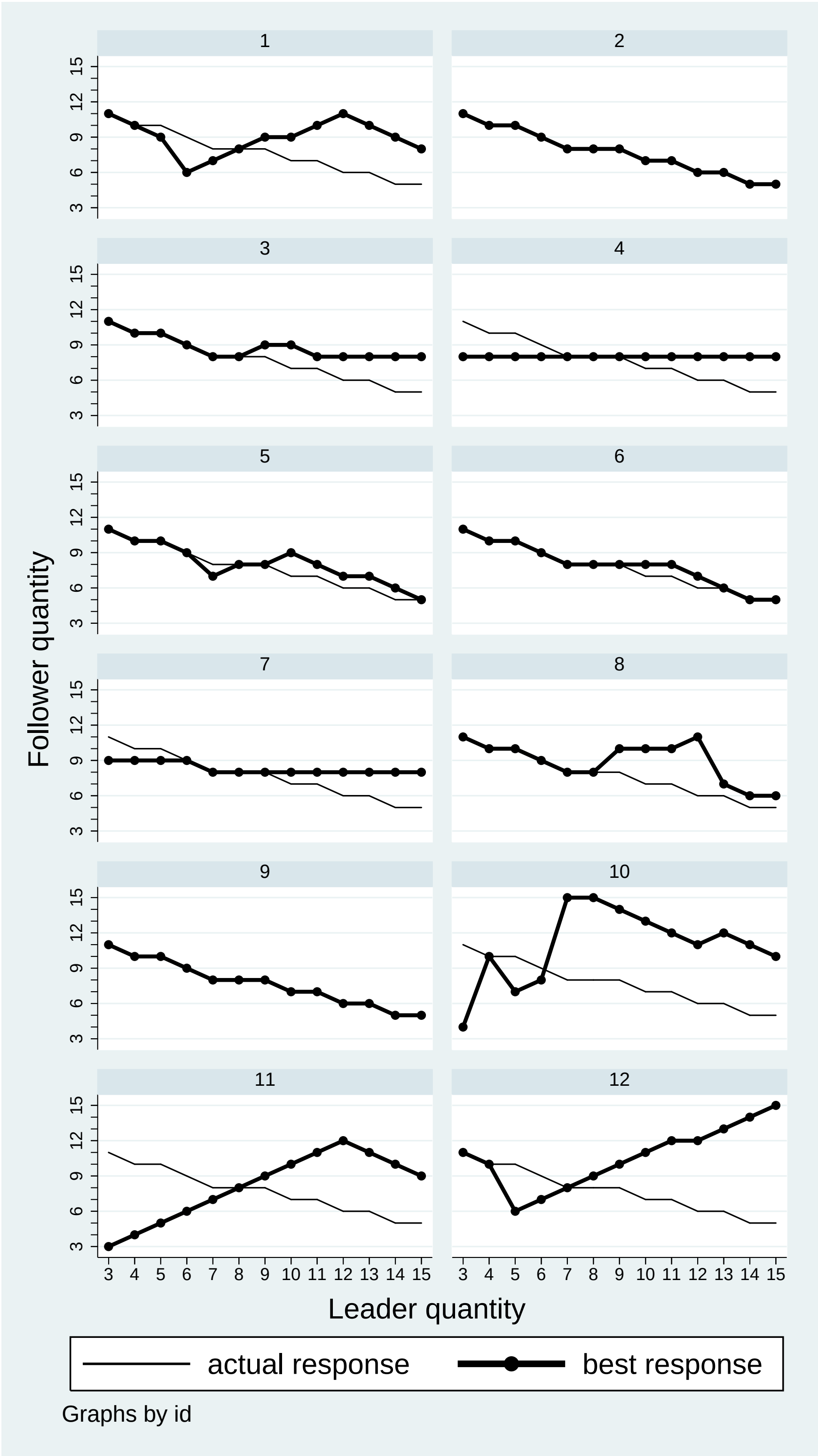
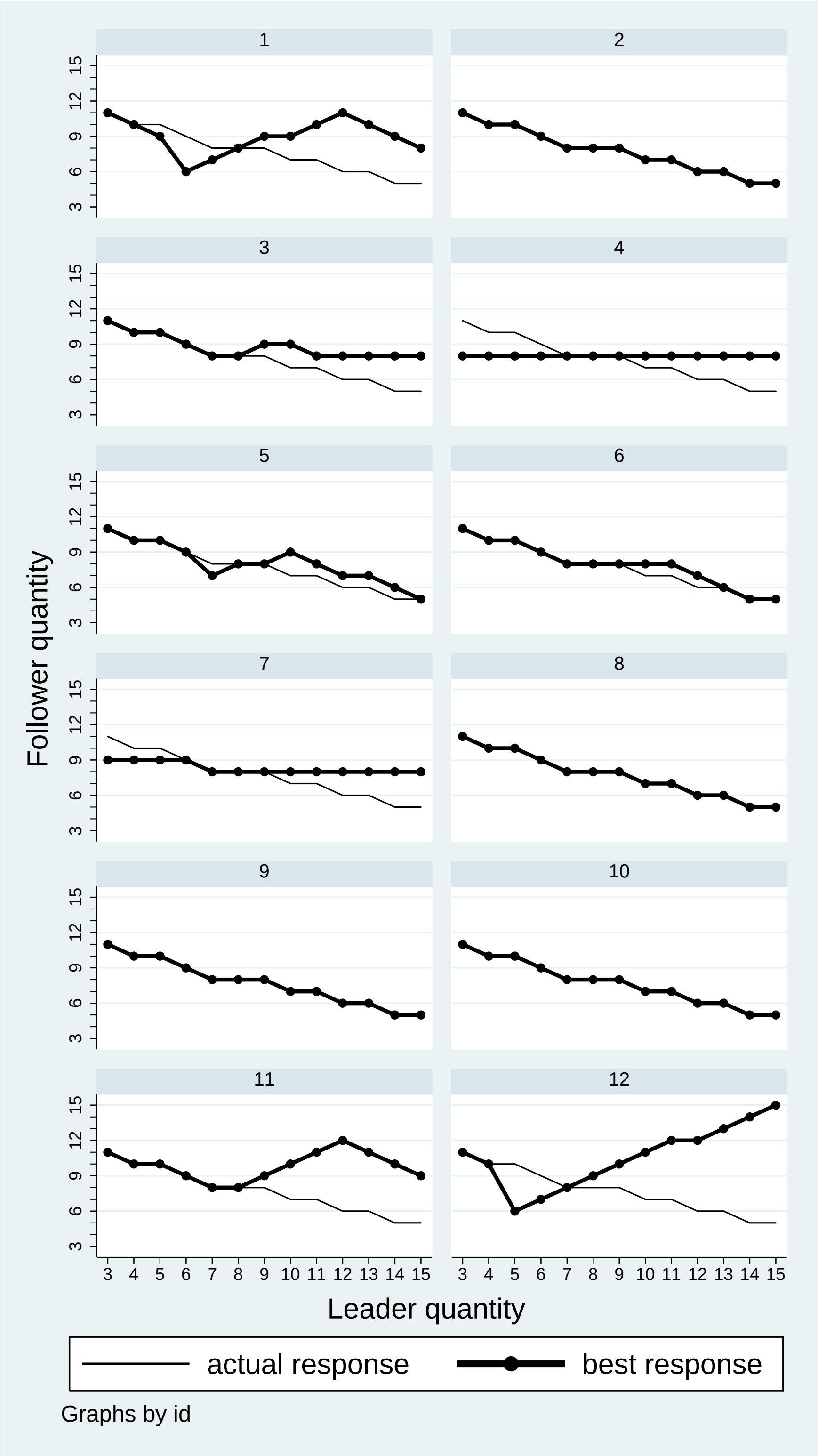


Figure 4.8: Treatment SM-TEAM: Response functions in round 15



Subject instructions

Please read these instructions carefully! Please do not talk to your neighbours and remain quiet during the entire experiment. If you have a question, please raise your hand. We will come up to you to answer it.

General information

In this experiment you can earn money by interacting with other participants. Your earnings will be measured in " Points". The number of Points that you can earn depends on the decisions that you and other participants make.

At the beginning of the experiment, every participant will receive 75 Points as an initial endowment. Your total number of Points at the end of the experiment will be equal to the sum of the Points you have earned in each round plus your initial endowment. For every 50 Points you will be paid 1 Euro in cash.

Description of the experiment

The experiment consists of 15 periods. You will act in the role of a firm which produces the same product as another firm in a market. There are two types of firms: A-firms and B-firms. In each period each A-firm will be randomly matched with a B-firm. Both firms have to decide which quantities they want to produce.

In the attached table, you can see the resulting profits of both firms for all possible quantity combinations. The table is read as follows: the head of each row represents an A-firm's quantity and the head of each column represents a B-firm's quantity. Inside the little box where row and column intersect, the A-firm's profit matching this combination of quantities stands up to the left and the B-firm's profit matching these quantities stands down to the right.

How are decisions made in each period? The procedure is that first the A-firm and then the B-firm decides. This means that the A-firm chooses its quantity first (selects a row in the table). Then the B-firm is informed about the A-firm's choice. Knowing the quantity produced by the A-firm, the B-firm then decides on its quantity (selects a column in the table).

[The following paragraph only in SM-TEAM *and* SM-IND *treatments]*

But the above procedure will be conducted in the following way: Instead of deciding one after the other, both firms decide about their quantities at the same time. But while the A-firm only has to choose one quantity, the B-firm has to make a number of conditional quantity choices. More precisely, for every possible quantity of the A-firm (i.e., for every row in the table), the B-firm has to choose a quantity (i.e., a column in the table). That is, the B-firm has to make "if-then decisions" of the form: "If the A-firm chooses quantity x, I (the B-firm) will choose quantity y." As there are thirteen possible quantities, the B-firm has to make thirteen decisions. This procedure corresponds to the one described above where the A-firm chooses its quantity first followed by the B-firm who chooses its quantity after being informed about the A-firm's quantity decision. This is so, since the B-firm has to decide how it would react to each possible quantity the A-firm can select. It is then possible to match the A-firm's quantity with the relevant quantity of the B-firm to determine the outcome in the market.

[The following four sections only in SM-TEAM *and* SEQ-TEAM *treatments]*

Acting in teams

You will be acting in teams. At the beginning of the experiment, the computer will randomly match you with two other participants and the three of you will act as a team throughout the experiment, either representing an A-firm or a B-firm.

What does it mean to act as a team? As a team you will make decisions jointly. That is, the three of you must decide together what choices to make (either as an A-firm or as a B-firm) and the payoffs of all three of you will depend on these choices. To facilitate team coordination, there will be a place on your screen to send messages back and forth to each other. Although we will record these messages, only you and your team members will see them. Think of the message space as your own private chat system to help you decide what to do. More on how this will work shortly. Note, in sending messages back and forth between you and your team members we request you follow three simple rules: (1) Discussion must be in English. No other language is allowed. (2) Be civil to each other, don't use bad language, and don't make any threats to each other. (3) Do not identify yourself, your seat number or anything that might reveal your identity. The communication channel is intended for you to use to discuss and coordinate your choices and should be used that way.

Description of the communication and decision-making screen

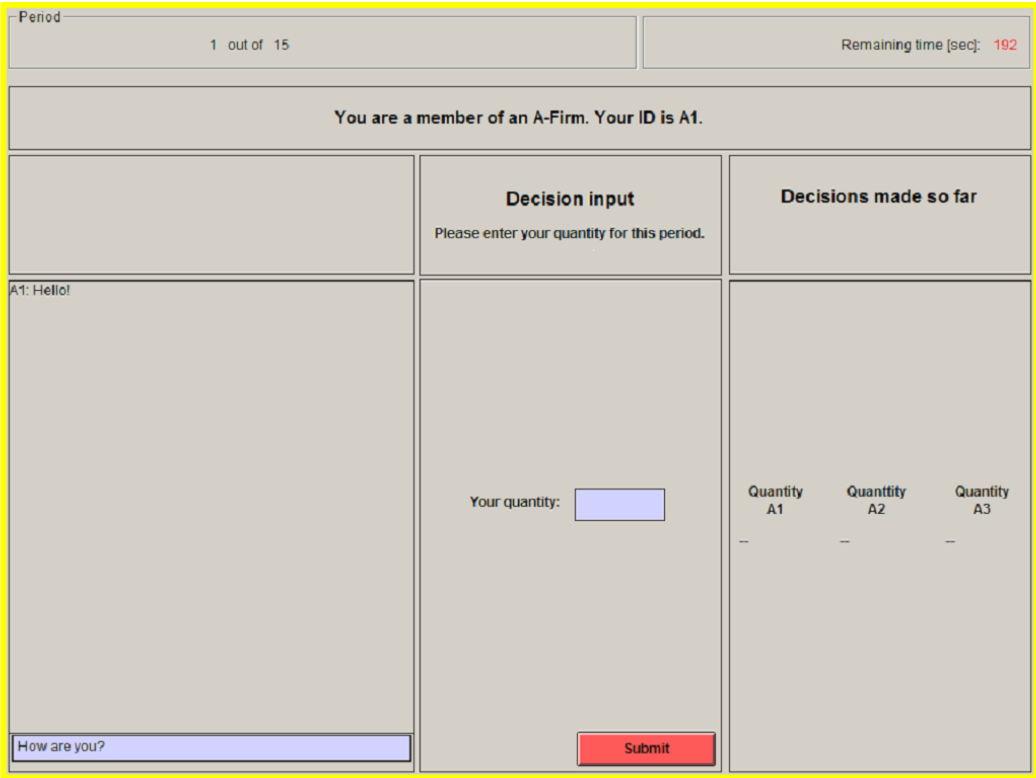
In the following we will describe the structure of the communication and decision-making screen that each member of an A-firm and each member of a B-firm will face during the experiment. Basically, for both a member of an A-firm and a member of a B-firm the screen consists of three boxes: the dialogue box, the decision-making box, and a box that shows which decisions have been made so far in a given period by all members of a team (which we call the "Decisions made so far" box).

Screen for a member of an A-firm

We will first describe the communication and decision-making screen for a member of an A-firm. This screen is shown on the next page. Imagine in what follows that you are a member of an A-firm.

The line on top of the screen indicates that this is a screen of a member of an A-firm. It also indicates the ID of this participant who is called "A1". The IDs of the other members of an A-firm are "A2" and "A3". Each member of an A-firm will be informed about his/her ID in this top line of the decision screen.

Figure 4.9: Screen shot for SM-Team first mover



The dialogue box is on the left hand side of the screen. If you click on to the lower (light grey) part of the box you can type a message to your team members (only you and your team members can see your messages, no other participants can see them). You can use this box to discuss what choice you want to make. To send a message hit the enter key. Both your messages and your team members' messages will be reported at the top of the dialogue box with the ID in front. In the sample screen on the next page, member A1 has already sent the message "Hello?" and has just typed the new message "How are you?"

The decision-making box is in the middle of this screen. Since as an A-firm you are acting as teams, you and your team members must coordinate on your quantity choice. As described above, members of an A-firm will have to coordinate on a single quantity they want to produce. To make a quantity decision, each member of an A-firm will have to type in the quantity you agreed upon using the dialogue box. You type in the quantity that you and your team members agreed on into the box in the middle of the decision-making box, followed by a click on the " Submit" button. The quantity you and your team members submit will immediately appear in the box on the right hand side of your screen which is called "Decisions made so far." (Look at the "Decisions made so far" box on the sample screen on the next page. The three

"–" below the IDs of the A-firm members in this box indicate that none of the team members of this particular A-firm has yet submitted a quantity.)

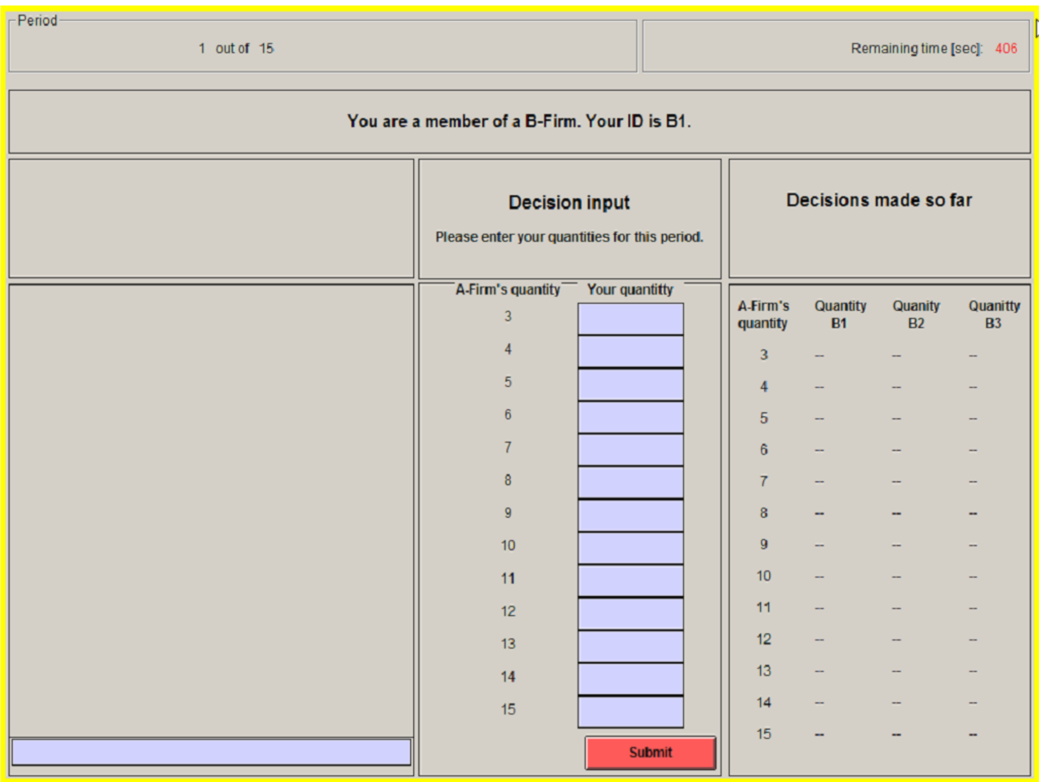
If the quantities you and your team members typed in are not the same, an error message will appear at the top of the "Decisions made so far" box, informing you that your and your team members' quantities do not match and that there is still disagreement. You can then use the chat box again to reach agreement. It might then be necessary that you revise your choice, type in a revised quantity in the decision-making box, and click the " Submit" button again.

If the quantities you and your team members typed in are the same, the decision screen will disappear and a message will indicate that your team has reached agreement.

Screen for a member of a B-firm

We will now describe the communication and decision-making screen for a member of a B-firm. This screen is shown on the next page. Imagine in what follows that you are a member of a B-firm.

Figure 4.10: Screen shot for SM-Team second mover



The line on top of the screen indicates that this is a screen of a member of a B-firm. It also indicates the ID of this participant who is called " B1" . The IDs of the other members of a B-firm are “ B2” and “ B3” . Each member of a B-firm will be informed about his/her ID in this top line of the decision screen.

The dialogue box is located on the left hand side of the screen and works as described above for a member of an A-firm. The only difference is, of course, that messages sent are displayed with the ID of a B-firm in front.

The decision-making box is in the middle of this screen. Since as a B-firm you are acting as teams, you and your team members must coordinate on your quantity choices. (The following sentences in parenthesis only in SM-TEAM treatment)(As described above, members of a B-firm will have to make contingent decisions specifying how they react to each possible quantity of the A-firm. The column on the left of the decision-making box, labelled “ A-firm’s quantity,” shows all possible quantities of the A-firm. Next to each of these quantities, in the column labelled “ Your quantity” you will have to type in the quantity with which you and your B-firm team members want to react to each of the A-firm’s quantities.) (The following sentence in parenthesis only in SEQ-TEAM treatment. Note that the screen shot in the SEQ-TEAM treatment for B firms is identical to the screen of A-firms except the labels.)(On top of the decision-making box the quantity chosen by the A-firm is indicated.) To make quantity decisions, each member of a B-firm will have to type in the quantities you agreed upon using the dialogue box. After typing the quantities, click the “ Submit” button. The quantities you and your team members submit will immediately appear in the box on the right hand side of your screen which is called “ Decisions made so far.” (The meaning of the Look at the “ Decisions made so far” box on the sample screen on the next page. The “ –” below the IDs of the B-firm members in this box indicate that none of the team members of this particular B-firm has yet submitted a quantity.)

If the quantities you and your team members typed in are not the same, an error message will appear at the top of the “ Decisions made so far” box, informing you that your and your team members’ quantities do not match and that there is still disagreement. You can then use the chat box again to reach agreement. It might then be necessary that you revise your choice(s), type in revised quantities in the decision-making box, and click the “ Submit” button again.

If the quantities you and your team members typed in are the same, the decision screen will disappear and a message will indicate that your team has reached agreement.

[The following subsection only for SM-IND *and* SEQ-IND *treatments]*

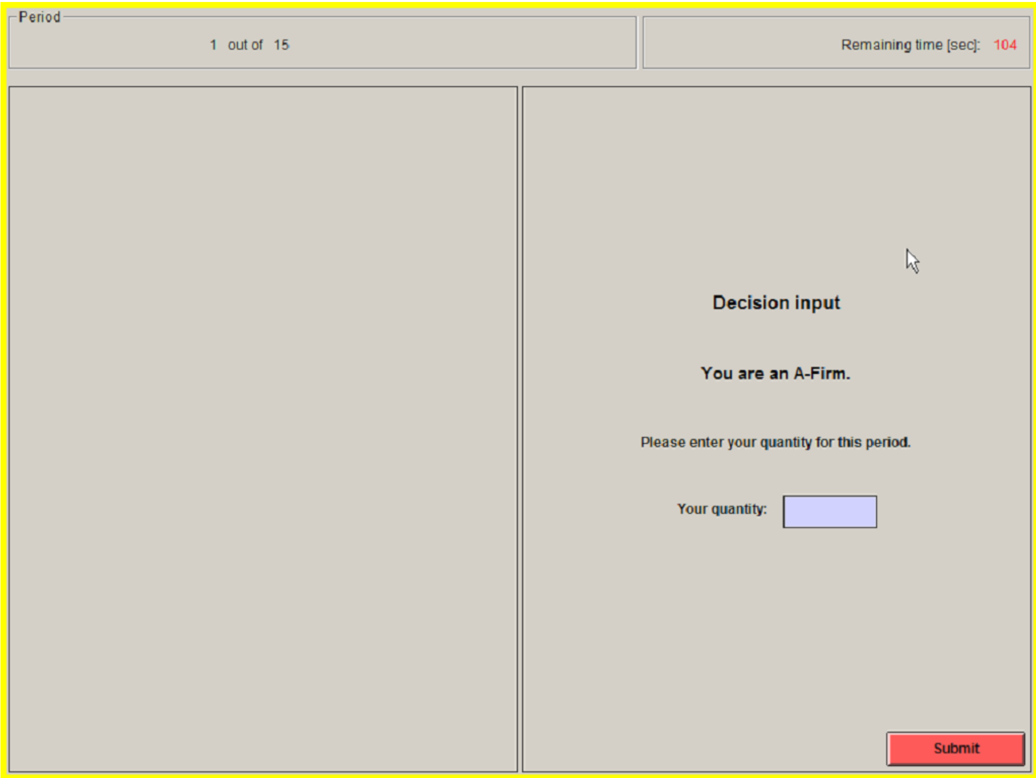
Description of the decision-making screen

In the following we will describe the decision-making screen that an A-firm and a B-firm will face during the experiment.

Screen for a member of an A-firm

We will first describe the decision-making screen of an A-firm. This screen is shown on the next page. Imagine in what follows that you are an A-firm.

Figure 4.11: Screen shot for SM-Ind first mover



The second line from above in the box on the right-hand side indicates that this is a screen of an A-firm. You type the quantity you want to choose into the box on the bottom of decision-making box, followed by a click on the "Submit" button.

Screen for a member of a B-firm

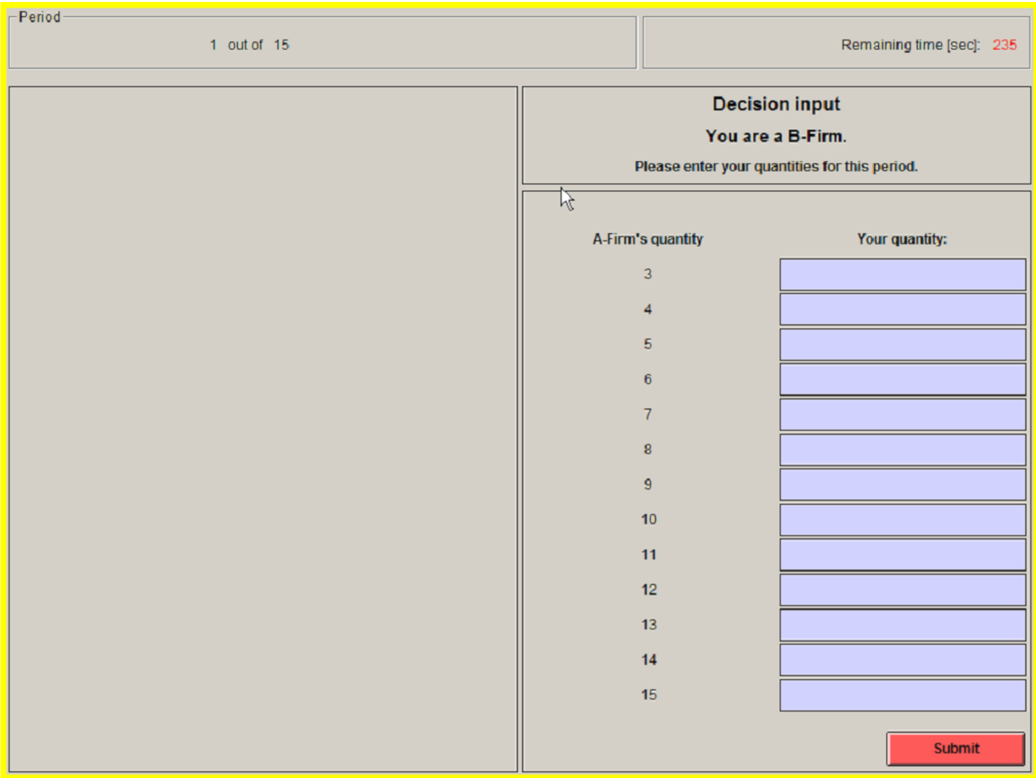
We will now describe the decision-making screen of a B-firm. This screen is shown on the next page. Imagine in what follows that you are a B-firm.

The second line from above in the box on the right-hand side indicates that this is a screen of a B-firm. (The following sentences in parentheses only in SEQ-IND treatment. Note that the screen shot in the SEQ-TEAM treatment for B firms is identical to the screen of A-firms except the labels.) (On the next line the quantity chosen by the A-firm is indicated. You type the quantity that you want to choose, followed by a click on the "Submit" button.)

[The following bullet list only in SM-IND *treatment]*

As described above, a B-firm will have to make contingent decisions specifying how it reacts to each possible quantity of the A-firm. The column on the left of the

Figure 4.12: Screen shot for SM-Ind second mover



decision-making box, labelled "A-firm's quantity" shows all possible quantities of the A-firm. Next to each of these quantities, in the column labelled " Your quantity" you will have to type in the quantity with which you want to react to each of the A-firm's quantities, followed by a click on the "Submit" button.

Payoffs, information during the experiment, and matching

Payoff in a period: Each member of an A-firm or a B-firm will earn the amount indicated in the table for the selected quantity combination of both firms.

At the start of a new period, all members of both firms will be informed about the quantity of the A-firm, the relevant quantity of the B-firm, and own profit in the previous period. When the experiment starts, you will be told on your computer screen whether you are a member of an A-firm or a B-firm. You will then keep this role during the entire experiment.

The following bullet list only in SM-TEAM *and* SEQ-TEAM *treatments*

Of the 18 participants in the room, 3 teams acting as A-firms and 3 teams acting as B-firms will be randomly formed at the beginning of the experiment. In each period, A-firms will be randomly matched with any of the B-firms in the room. You will not know the identity of the other firm (and its team members) you are matched with in any period. Remember, that the composition of all teams of 3 participants each remains fixed throughout the entire experiment.

The following bullet list only in SM-IND *and* SEQ-IND *treatments*

Of the participants in the room, groups of 6 participants each will be randomly formed at the beginning of the experiment. (The composition of these groups of 6 participants each will remain the same throughout the entire experiment.) In each group, 3 participants will act as an A-firm and 3 participants will act as a B-firm. In each period, A-firms of a group will be randomly matched with any of the B-firms of the same group. You will not know the identity of the other firm you are matched with in any period.

Table 4.11: Payoff-table

Quant.	3	4	5	6	7	8	9	10	11	12	13	14	15
3	54 / 54	51 / 68	48 / 80	45 / 90	42 / 98	39 / 104	36 / 108	33 / 109	30 / 110	27 / 108	24 / 104	21 / 98	18 / 90
4	68 / 51	64 / 64	60 / 75	56 / 84	52 / 91	48 / 96	44 / 99	40 / 100	36 / 99	32 / 96	28 / 91	24 / 84	19 / 75
5	80 / 48	75 / 60	70 / 70	65 / 78	60 / 84	55 / 88	50 / 89	45 / 90	40 / 88	35 / 84	29 / 78	25 / 70	20 / 60
6	90 / 45	84 / 56	78 / 65	72 / 72	66 / 77	60 / 80	54 / 81	48 / 80	41 / 77	36 / 72	30 / 65	24 / 56	18 / 45
7	98 / 42	91 / 52	84 / 60	77 / 66	70 / 70	63 / 72	55 / 71	49 / 70	42 / 66	35 / 60	28 / 52	21 / 42	14 / 30
8	104 / 39	96 / 48	88 / 55	80 / 60	72 / 63	64 / 64	56 / 63	48 / 60	40 / 55	32 / 48	24 / 39	16 / 28	8 / 15
9	108 / 36	99 / 44	89 / 50	81 / 54	71 / 55	63 / 56	54 / 54	45 / 50	36 / 44	27 / 36	18 / 26	9 / 14	0 / 0
10	109 / 33	100 / 40	90 / 45	80 / 48	70 / 49	60 / 48	50 / 45	40 / 40	30 / 33	20 / 24	10 / 13	0 / 0	−10 / −15
11	110 / 30	99 / 36	88 / 40	77 / 41	66 / 42	55 / 40	44 / 36	33 / 30	22 / 22	11 / 12	0 / 0	−11 / −14	−22 / −30
12	108 / 27	96 / 32	84 / 35	72 / 36	60 / 35	48 / 32	36 / 27	24 / 20	12 / 11	0 / 0	−12 / −13	−24 / −28	−36 / −45
13	104 / 24	91 / 28	78 / 29	65 / 30	52 / 28	39 / 24	26 / 18	13 / 10	0 / 0	−13 / −12	−26 / −26	−39 / −42	−52 / −60
14	98 / 21	84 / 24	70 / 25	56 / 24	42 / 21	28 / 16	14 / 9	0 / 0	−14 / −11	−28 / −24	−42 / −39	−56 / −56	−70 / −75
15	90 / 18	75 / 19	60 / 20	45 / 18	30 / 14	15 / 8	0 / 0	−15 / −10	−30 / −22	−45 / −36	−60 / −52	−75 / −70	−90 / −90

Note: The head of the row represents one firm's quantity and the head of the column represents the quantity of the other firm. Inside the box at which row and column intersect, one firm's profit matching this combination of quantities stands up to the left and the other firm's profit stands down to the right. As mentioned in the text, 14 entries were manipulated in order to get unique best replies.

Table 4.12: List of statements in follower discussions in SEQ-TEAM

Categories of motives mentioned in follower group discussions	Leaders' Choices Q=6	Q=7	Q=8	Q=9	Q=10	Q=11	Q=12
	12	42	79	4	14	9	20
Best-response motives							
Proposal to play best response	11	24	72		4	4	9
Suggesting payoff maximization	5	5	6		2	2	3
Suggesting payoff maximization because of random matching	1	1			1	1	
Avoid punishment because of loss					1	2	3
Raising doubts on cooperative quantity because of random matching	1	1					
Raising doubts whether punishment is the right to do because of its questionable effect on leader firm given random matching (" is punishment worth it")			1		3	2	2
(Far-sighted) Strategic-teaching motives							
Proposal to collude because of long-term perspective	4	5	2				
Proposal to collude because it does not cost too much in the current period	4	4					
Proposal to collude because of high probability of meeting same leader again	1	1					
Proposal to punish because of long-term perspective				2	1	2	1
Suggesting that own payoff maximization might induce the leader to raise their quantity	1	3			2	2	3
Suggesting punishment because it is not too costly							4
Raising doubts whether payoff max. is the right thing to do in terms of long-term profit							1
Other-regarding motives							
Judging the leader's choice/ leader group itself as "unfair," "bad", "annoying," "greedy"			2		5	2	7
Punishment motive: Don't make firm a comfortable					1		2
Judging the leader to be a " good company"	6	2	3				
Motivation: trust and trust-worthy		3					
Motive: punishment makes the leaders lose more than followers themselves					2	3	5
Motive: fairness (profit sharing)	4	1	2		1		2

continued

Categories of motives mentioned in follower group discussions	**Leaders' Choices**						
	Q=6	**Q=7**	**Q=8**	**Q=9**	**Q=10**	**Q=11**	**Q=12**
Unidentifiable motives							
Proposal to cooperate	8	32	1	1	1		
Proposal to punish			3	4	14	7	21
Pointing at the fact that punishment comes with own loss				2	2	1	
Raising doubts on fairness	1		1			1	
Motive: pareto optimality	1						
Motive: putting themselves into the shoes of the leader		3	1	1	1		2
Raising the question about motive behind the leader´s quantity choice	1	2	1		2		2
Suggesting that Cournot outcome might be " best"			1		2	1	1
Suggesting that collusive outcome / cooperation might be "best"	1	2	2			1	1
Suggesting Stackelberg outcome as the result of (correct) backward induction.	1						2
Raising doubts whether payoff maximization is the right to do							1
Complaining about being in the disadvantaged player position					1	2	3

Note: Unidentifiable motives are those that can not be anambiguously be assigned to any of the other categories listed in this table. Entries in the table are numbers of observation.

Table 4.13: Motives mentioned in follower discussions in SEQ-TEAM

Categories of motives mentioned in follower group discussions	Overall characterization of a round's discussion: Quick agreement on R	Quick agreement on PM	Quick agreement on P	PM vs R, R wins	PM vs R, PM wins	PM vs P, P wins	PM vs P, PM wins	How much P?
	23	90	15	10	5	20	7	10
Best-response motives								
Proposal to play best response		90		11	5	11	7	
Suggesting payoff maximization		4		5	3	7	4	
Suggesting payoff maximization because of random matching					1		3	
Avoid punishment because of loss							3	3
Raising doubts on cooperative quantity because of random matching				2				
Raising doubts whether punishment is the right to do because of its questionable effect on leader firm given random matching (" is punishment worth it")						5	3	
(Far-sighted) Strategic-teaching motives								
Proposal to collude because of long-term perspective				7	4			
Proposal to collude because it does not cost too much in the current period				8				
Proposal to collude because of high probability of meeting same leader again				2				
Proposal to punish because of long-term perspective						4		1
Suggesting that own payoff maximization might induce the leader to raise their quantity	1		1	1	5	2	1	
Suggesting punishment because it is not too costly						4		
Raising doubts whether payoff maximization is the right to do in terms of long-term profit				1				

to be continued (see next page)

continued

Categories of motives mentioned in follower group discussions	Quick agreement on R	Quick agreement on PM	Quick agreement on P	PM vs R, R wins	PM vs R, PM wins	PM vs P, P wins	PM vs P, PM wins	How much P?
Other-regarding motives								
Judging the leader's choice/ leader group itself as "unfair", "bad", "annoying", "greedy"			10			3	1	2
Punishment motive: Don't make firm a comfortable			3					
Judging the leader to be a " good company"	4	3		2	2			
Motivation: trust and trust-worthy				2	1			
Motive: punishment makes the leaders lose more than followers themselves						7	2	1
Motive: fairness (profit sharing)	1			5		3	1	
Unidentifiable motives								
Proposal to cooperate	30			7	6			
Proposal to punish			13	4		16	6	10
Pointing at the fact that punishment comes with own loss								2
Raising doubts on fairness						2	1	
Motive: pareto optimality				1				
Motive: putting themselves into the shoes of the leader	3		2			1	1	1
Raising the question about motive behind the leader's quantity choice	1		2			2	1	2
Suggesting that Cournot outcome might be " best"	1	2				1	1	
Suggesting that collusive outcome / cooperation might be "best"		5			2			
Suggesting Stackelberg outcome as the result of (correct) backward induction.	1				2			
Raising doubts whether payoff maximization is the right to do								
Complaining about being in the disadvantaged player position		1	1	1		2		1

Notes: Abbreviations used: R = Reward, PM = Profit maximization, P = Punishment. In case there are two motives, the motive marked in bold font is the " winning" motive. Unidentifiable motives are those that can not be anambiguously be assigned to any of the other categories listed in this table. Entries in the table are numbers of observation.

CHAPTER 5

PUNISHMENT IN A HETEROGENOUS PUBLIC GOODS GAME [1][2]

This chapter is published in *De Economist* (2008) 156: 269-293.

5.1 Introduction

The ever-increasing expansion of knowledge in day-to-day life has resulted in greater demand for cooperation, and teamwork is increasingly being perceived as a crucial part of being successful. When some members of a group driven by self-interest withhold their effort contributions, the enforcement of sanctions is a common means through which people retaliate against perceived injustices. Examples of sanctions include fines and restrictions that may be implemented by a legal system, or costs of money and time that may be imposed by private individuals on the offender (Masclet et al. (2003)).

Sanction behavior has been investigated extensively via a laboratory experiment called Voluntary Contribution Mechanism (VCM), or Public Good Game (PGG). In this game, every member of the group receives an initial endowment of money, and members have to choose simultaneously the proportion of money they keep for themselves and the proportion of money to be placed in a public account that would be of benefit to everyone. Each member, after observing the individual contribution of all the others, has an opportunity to reduce the earnings of any group member at his or her own cost. Given the parameters set in VCM, the dominant strategy for a participant is to have zero contribution and to never punish. Nevertheless, most

[1] This chapter is based on Tan (2008).

[2] I am grateful to Professor Charles Noussair, Jan Potters, Urs Fischbacher, the anonymous referee, and participants at the NAKE research day 2007 in Utrecht, the Netherlands, for their valuable comments and suggestions. I would also like to thank CentER Lab for financial support.

public good games with punishment observe a prevalent use of sanctions. These credible punishment threats sustain cooperation effectively (Yamagishi (1986); Fehr and Gächter (2000) and Fehr and Gächter (2002); Bowles et al. (2001); Masclet et al. (2003)).

So far, the punishment literature on VCM has focused mainly on symmetric players having the same impact on the group account in the contribution stage. In most real-world scenarios, however, symmetric players are the exception rather than the norm, while the asymmetry across different group members is a feature of many situations of interest. A common example that comes to mind involves a group of students working together on a project. For some students, less effort is needed to achieve the same progress than is the case for others, due to their relatively higher abilities or richer experience. Similarly, larger firms in a cartel of a certain industry are more likely to have stronger bargaining power in negotiations with the government because of their size and capacity in market share. In this paper, a situation in which different group members have asymmetric impacts on a collective goal is referred to as "productivity heterogeneity". The aim of this study is therefore to investigate the efficiency of the sanction system under productivity heterogeneity: if individual members of a group of agents with asymmetric productivities are allowed to express disapproval against the actions of each other, then who determines the punishment and who gets punished? Do behavioral differences occur when various members react to punishment? To the best of my knowledge, this is the first study on this topic.

A starting point for the analysis will be the selection of an appropriate proxy for productivity. A key parameter in the public good game, marginal per capita return (MPCR), is the benefit that each participant receives from each money unit contributed to the group account by any group member. Since a higher MPCR reduces the cost of contributing to the group account and consequently may induce an increase in contributions, it may be considered as a proxy for the productivity of group subjects (Isaac and Walker (1988)). Hence, the higher the productivity of a subject (which is equivalent with the case of a higher MPCR), the less effort it takes that subject to contribute a given amount of output. Hereafter, the term MPCR will be used interchangeably with productivity.

To implement this experiment, the study observes cooperation and punishment behavior in a treatment with heterogeneous agents (where agents have different MPCRs to the public account) and compares the results to the existing literature. Since little analysis has been carried out to explore the effect of productivity heterogeneity, we will attempt to fill the gap by also analyzing a treatment with heterogeneous agents but with no sanction opportunities. [3] The effect of productivity heterogeneity can therefore be measured by comparing this treatment with a symmetric one (in which

[3] Although Fisher et al. (1995) probe the effect of asymmetric MPCR, their study differs from ours in terms of baseline comparison and information structure.

all agents have the same MPCR), controlling for average productivity.

The remainder of the chapter proceeds as follows. Section 5.2 presents public good game experiments related to heterogeneity and punishment. Section 5.3 derives hypotheses according to the existing literature. Section 5.4 describes the experiment implementation, and Section 5.5 illustrates the results. Section 5.6 provides concluding remarks.

5.2 Literature review

Many studies have uniformly found that players in VCM games exhibit a substantially higher rate of cooperation than can be expected under the assumption of the standard economic model of the self-interested actor. Contribution rates of 40%-60% are reported by many studies, but they decrease with repetition (Kim and Walker (1984); Isaac et al. (1985); Andreoni (1988); Isaac and Walker (1988); Weimann (1994)).

5.2.1 Literature on heterogeneity

There are several ways to generate asymmetry in VCM. The first way is to allow variation of MPCR. Many public good studies have found that a higher MPCR reduces the contribution cost, which implies a higher chance of contribution. Ledyard (1995), surveying the literature on public good games, concludes that MPCR has a strong positive effect on contribution rates.

To the best of my knowledge, most of the experiments addressing productivity variation are not linear public good games (Marwell and Ames (1979), Marwell and Ames (1980); Bagnoli and McKee (1991). Fisher et al. (1995) is the only study that examines exclusively the effect of asymmetric MPCR structure based on a linear public good game. In their experiment, subjects are separated into groups of four - with two high MPCR (0.75) and two low MPCR (0.3). A total of twenty periods are divided into two stages, and the MPCRs of all subjects are shifted from one to the other after the tenth period. They conclude that the subjects seem to focus only on their own MPCR, for whoever is assigned MPCR 0.75 contributed more than the one who is assigned MPCR 0.3. This reduces the group average contribution in an asymmetric group to an intermediate level between baseline groups featuring a high- and a low MPCR. Their experiment differs from ours in two respects. First, their comparisons are based on the average contribution level of a mixed group and that level is the weighted average of two homogeneous groups with extreme MPCRs. This study, in contrast, is based on a mixed group and a homogeneous group with the same average MPCR. The second difference is that the information structures are different in two settings. In Fisher et al. (1995), participants are not told explicitly that different individuals necessarily have different MPCRs, but simply that the MPCR are not necessarily the same. The MPCR information is also private when the experimenters inform the subjects about the MPCR changes. This experiment, however, makes productivity distributions explicit among all players so as to allow richer reciprocal interactions. Ledyard (1995) gives a tentative conjecture that productivity heterogeneity lowers the rate of contribution, unless there is incomplete information and no repetition.[4]

Apart from the asymmetric payoff structure, another way to generate heterogeneity is by varying the endowments of the players. Unlike the studies on productivity, the results of these studies on endowment heterogeneity are contradictory and nonconclusive. A number of studies report that asymmetric endowments decrease the cooperation level, especially when all players know the asymmetry (Anderson et al. (2004); Cherry et al. (2005); Oxoby and Spraggon (2006)). Nonetheless, several other studies find that endowment asymmetry does not so much hamper as promote contribution (Chan et al. (1996); Buckley and Croson (2006)).

In addition to the studies focusing exclusively on productivities and endowments, heterogeneity can also be generated by other means. Some studies interact both forms of heterogeneity under different information structures (Chan et al. (1999)), some generate asymmetries in the dynamics of the game by introducing leaders (Potters et al. (2007)), and a few other experiments do the same by varying private account valuations across agents (Palfrey and Prisbrey (1997)). Due to different game settings, the results differ significantly and cannot be compared directly with each other.

5.2.2 Literature on sanctions

Sanction is considered to be one of the most robust mechanisms for eliciting contributions. The earliest studies on sanctioning systems under VCM are carried by social psychologists (Yamagishi (1986)) and political scientists (Ostrom et al. (1992)). They find that this mechanism can be self-maintained, and that it serves to increase contribution levels.

An influential economic experiment conducted by Fehr and Gächter (2000) (hereafter FG), devises a situation in which the possibility of strategic punishment can be removed. In the first stage of the experiment, four subjects play a standard public good game. In the second stage, each subject, after reading the contributions and earnings of the all group members, is given the chance to reduce the earnings of any other player or players in his or her group by using his or her earnings in the contribution stage. Game theory predicts that the unique sub-game perfect equilibrium to this finite two-stage game is that agents never punish in the second stage because any punishment will be costly for them. Since the threat of punishment is not credible, no one will contribute to the group account in the first stage. However, FG observe a significant positive amount of punishment mete out by subjects, and free riding is remarkably curtailed accordingly. This result is obtained under both Partner Treat-

[4]Ledyard also points out that the effect of heterogeneity on cooperation is quite weak: "there does not yet appear to be enough evidence for acceptance. Quite often there is conflicting evidence."

ment (the four subjects remain in the same group for all ten periods) and Stranger Treatment (the subjects are randomly reassigned after each round). Many subsequent studies replicate the experiment and obtain similar results (Bowles et al. (2001); Fehr and Gächter (2002); Masclet et al. (2003); Noussair and Tucker (2005); Sefton et al. (2007)).

There have been many extensions of FG on varying parameters of a VCM game. For instance, Bowles et al. (2001) extends FG's experiment to measure the sanctioning effect under different group sizes and MPCRs to the public account. Their data suggest that the amount of punishment received by free riders is increasing in both group size and MPCR. As a result, the contribution levels of large groups with a high MPCR are very high when punishment is allowed. As an extension of Bowles et al. (2001), Carpenter (2007) systematically examines the interaction of group size, MPCR and monitoring technology. He reports that punishments are sensitive to group structures. Sefton et al. (2007) extends FG's experiment by adding a Reward Treatment (where players can give a bonus to others based on contribution) and a Combined Treatment (where players can both award and punish each other), and shows that the Combined Treatment works most effectively in promoting contributions. Asymmetric sanctioning power is investigated recently. Nikiforakis et al. (2010) discovers that asymmetries in the distribution of punishing abilities seem to have no effect on either the level or the evolution of cooperation over time. What increases cooperation levels is the higher average ability to punish.

Sanction motivation is another interesting issue. Researchers have discovered that punishments are not merely monetary based; the expression of disapproval itself is also found to be effective. Masclet et al. (2003) and Noussair and Tucker (2005) provide evidence of cooperation being enhanced purely by "cheap talk"; that is, the opportunity for agents to express disapproval of other's decisions without deducting their monetary earnings. Likewise, Carpenter et al. (2004) conducts a field experiment by imposing cost to punishers but keeping at zero the material harm to those who were punished. This study discovers that the sanction mechanism enhances cooperation in only one of the two samples.

While punishment tends to increase cooperation, it is not a panacea: if individuals are given the possibility to counter-punish, then cooperation quickly breaks down (Denant-Boemont et al. (2007); Nikiforakis (2008)). In addition, Bochet et al. (2006) reports that punishment, while discouraging free riding, does not raise contribution as efficiently as communication does. Note that all of the above-mentioned studies on punishment assume that players have symmetric impacts on the group account in the contribution stage.

5.3 Preliminary conjectures

The literature mentioned in Section 5.2 cannot provide any comparable results on a heterogeneous and homogeneous group with the same average MPCR for this experiment. The only reliable clue available to us is a pattern in Fisher et al. (1995) that shows that the average group contribution of asymmetric groups is always between those of the high- and low-MPCR baseline groups. Since MPCR links positively with contribution, the contributions of two groups with the same average productivity can reasonably be expected to be similar.

Hypothesis 5.1 *(Heterogeneous Productivity Hypothesis): Without sanctioning, the group average contribution of a heterogeneous group is the same as that of a homogeneous group with the same average MPCR.*

Behavioral differences between the types are found in most of the above-mentioned studies. High MPCR are usually associated with higher contribution levels, and vice versa. For example, the discussion in Marwell and Ames (1979) reports that the "low interest" subjects are expected to under-contribute to the public good. Isaac et al. (1985) observes that individuals in high payoff conditions contribute more than individuals in low payoff conditions. Similarly, Fisher et al. (1995) find that high MPCR types, on average, contribute more than low-MPCR types do, in every period. We therefore propose the following hypothesis:

Hypothesis 5.2 *(Productivity Determinant hypothesis): In the absence of sanction opportunities, individual contribution increases when a player faces a higher MPCR.*

To the best of our knowledge, no previous study has explored punishment under productivity heterogeneity. Although several variants of sanction mechanisms could be used, this paper, as a tentative exploration, replicates only the canonical form of the punishment mechanism in FG's experiment. Based on the robust findings in previous experiments, punishment institution is anticipated as also being effective in terms of fostering cooperation in a heterogeneous game.

Hypothesis 5.3 *(Monetary Punishment Hypothesis): In the presence of heterogeneity, the opportunity for agents to reduce the monetary payoff of others after observing their decisions increases group average contribution levels.*

If the punishment institution were effective, who would become the enforcers in shaping players' cooperation? Olson (1971) proposed a "dominant power" would have a positive effect on cooperation. This means players have stronger incentives or power to provide the social goods will punish more. According to Fehr and Gächter (2002), 74.2 percent of these punishments are executed by above-average contributors to below-average contributors. Combining what Hypothesis 5.2 (that contribution rises when a player has a higher MPCR), we can see that low-MPCR players are expected to be the free riders while high-MPCR players are expected to be the "dominant power", or cooperation enforcers in the group.

Hypothesis 5.4 *(Punishment Enforcement Hypothesis): Given sanction opportunities, punishments are sent by high-MPCR subjects to low-MPCR subjects.*

However, the situation at each given individual contribution level will be different. Bowles et al. (2001) analyzes the way in which punishment affects individual contributions, and finds that the punishment a player receives from other group members increases saliently when the MPCR of the group account rises. Even though all players in this game are symmetric in productivity, similar results can be expected in this study. The logic behind this is natural: it is cheaper to spur on members to make additional contributions when their impact on the group account return increases, since every single contribution will give rise to more positive externalities to all group members. Therefore, Hypothesis 5.5 is given as follows:

Hypothesis 5.5 *(Conditional Punishment Hypothesis): Conditional on an individual contribution, high MPCR subjects receive more punishment.*

The experiment is designed to test cooperation and sanction behavior under productivity heterogeneity in a four-person linear public good game. The treatment variables are MPCR and the introduction of a sanction mechanism. The baseline Treatment, Treatment 1 (T1) replicates the standard VCM with a homogeneous MPCR 0.6 of all group members. Treatment 2 (T2) is identical to T1, except for one difference. Instead of endowed all subjects with the same MPCR, half of the group members have a high MPCR of 0.9, and the other half have a low MPCR of 0.3. T2 is used as a control group for Treatment 3 (T3). In T3, a sanction mechanism is added on top of T2. T3 is the key treatment for studying the effect punishment brings to cooperation level in a heterogeneous group, controlling for the average productivity.

5.4 The experiment

The experiment consisted of nine sessions conducted at CentER Lab, Tilburg University, located in Tilburg, the Netherlands. Each treatment, T1, T2 and T3, was in effect in three of the sessions. Seventy-two subjects, among whom 50% were females, were recruited via email contact from an experiment candidate list provided by CentER Lab. Subjects who majored in graduate level Economics were excluded. Some of the subjects had previously participated in economic experiments, but all were inexperienced with voluntary contributions mechanism. Each subject participated in only one session of the study. Since the student population in the contact list was very large (about 1400), the subjects were unlikely to know each other. The experiment was programmed and conducted with the software z-Tree (Fischbacher (2007)), developed at the Institute for Empirical Research in Economics at the University of Zurich.

Each session included eight participants that were separated into two groups of four. Before the start of each session, the computer program randomly designated the subjects into different groups according to their choices of terminal upon entering the room for the session. All three treatments adapted Partner Matching protocol, under which group assignments remained constant throughout the experiment.[5] A session consisted of 15 rounds. All 15 rounds of play counted towards final earnings, and there were no practice periods at the beginning of the sessions. At the beginning of each period every player was randomly given a number between 1 and 4 to distinguish their actions from those of the others during that period. To prevent the formation of individual reputation, the numbers were randomly reallocated at the beginning of every period.[6] In addition, participants in T2 and T3 were also informed of their productivity levels at the beginning of the experiment. These roles remained fixed for the duration of the experiment.[7] The above settings were common knowledge to all participants.

The instructions used in the experiment were modified on the basis of those used in Noussair and Tucker (2005). During each round, every subject was endowed with 10 tokens, with a conversion rate of 25 tokens = 1 Euro. Subjects simultaneously chose the number of tokens to keep for themselves and to put in the public account. T3 added a punishment stage after contribution decision in which subjects decided whether or not to register disapproval of each group member's decision by sending points to them. The entire experiment was done by subjects anonymously interacting with each other without informing the identity of the other group members, and communication was strictly forbidden all the time.

5.4.1 Treatment 1 (T1)

In the baseline treatment T1, the MPCR of all group members was 0.6, which means for each token put into the group account yields a payoff of 0.6 token to every members in group. The rest of tokens kept by the subjects they were added up to their private accounts. Therefore, the income in each round was calculated as:

[5]The purpose of choosing partner-matching protocol in this study is to simulate reality in which the members of a project team or a union working towards a common goal do not vary within a certain time period.

[6]Such a mechanism ensures that, even though the group members remain the same, the participants cannot link the actions of the other subjects across the periods. Thus, retaliation as in Denant-Boemont et al. (2007) and Nikiforakis (2008) is not possible.

[7]We used neutral language in the experiment. Players with MPCR 0.9 were "type A" and players with MPCR 0.3 were "type B". Moreover, sensitive terminologies such as "contribution" and "punishment" were avoided so as not to create biased decisions. For example, punishment was termed as "points that reduce another player's income".

$$I_i = 10 - C_{ij} + 0.6 \times \sum_{k=1}^{4} C_k \tag{5.1}$$

Where C_i is the contribution of agent i, and $\sum_{k=1}^{4} C_k$ is the sum of contributions to group account by all the members in the group, including agent i. After finishing making decisions, subjects were shown the contribution and earnings of every group member for every time period.

5.4.2 Treatment 2 (T2)

Heterogeneous productivity was generated in T2 by randomly assigning half of the group members a high MPCR of 0.9 and the other half a low MPCR of 0.3. The two MPCR levels were chosen such that the average MPCR of four group members equaled to 0.6, which was comparable with T1. The income calculation from the private account remained unchanged as T1, but the income from the group account was calculated as 90 percent of the total input of group members with MPCR 0.9 to the project plus 30 percent of the total input of group members with MPCR 0.3 to the project. That is, the income in each round in T2 should be:

$$I_i = 10 - C_i + 0.9 \times \sum_{h=1}^{2} C_h + 0.3 \times \sum_{l=1}^{2} C_l \tag{5.2}$$

Where C_i is the contribution of agent i (no matter what MPCR he or she has), and the third and fourth items are the sum of contributions of two high and low MPCR subjects to the group account respectively (agent i included). Since the income calculation was more complicated than T1, this calculation was posted together with contributions and earnings of his peers for each round. Note that even though this income calculation was different from homogeneous case, both games had the same unique sub-game perfect Nash Equilibrium of zero contribution for all players, if the games were finite repeated.

5.4.3 Treatment 3 (T3)

T3 was divided into two stages. The first stage was a replication of T2. After contribution decisions were made, sanction mechanism was added in the second stage. At the beginning of the second stage, the experimenter informed all subjects of the amount each of the other three members of his group contributed. Subjects were asked to send points ranging from 0 to 10 to every group member if they wished to. The random ID assignment setting mentioned above made it difficult to track an individual subject's contribution decision from one period to the next, or to target him specifically for punishment beyond the current period. Every point one subject sent reduced his/her earnings by 1 token and reduced the earnings of the participant receiving it by 2 tokens. Agent i's earnings in T3 thus became:

$$I_i = 10 - C_i + 0.9 \times \sum_{h=1}^{2} C_h + 0.3 \times \sum_{l=1}^{2} C_l - \sum_{k \neq i} P_{ik} - 2 \times \sum_{k \neq i} P_{ki} \tag{5.3}$$

Where $\sum_{k \neq i} P_{ik}$ is the sum of points agent i sent all group members, and $\sum_{k \neq i} P_{ki}$ is the sum of points agent i received from all other subjects. Again, the sub-game perfect equilibrium of this finite repeated game is nobody punishes.

At the end of the second stage, the computer displayed the subject's own type, the tokens he or she and all group members put into the project, the total number of points he or she received and assigned to others, the income of this round and its calculation. However, subjects were neither informed about the punishment information of other members, nor did they know the individual punishment sent by a specific player.

On average, a session lasted 50 minutes (including initial instruction and payment of the subjects) and a subject earned an average of 215.55 tokens (approximately 8.62 euros). Table 5.1 summarizes the structure of the experiment as a whole.

Table 5.1: Summary of treatment designs

Treatment names	Section number	Subject number	Group number	MPCR	Punishment allowed
T1	3	24	6	All equal to 0.6	No
T2	3	24	6	Half equal to 0.9; Half equal to 0.3	No
T3	3	24	6	Half equal to 0.9; Half equal to 0.3	Yes

5.5 Results and interpretation

5.5.1 Treatment effects

The focus of this section is group behavior. The principal research questions involve pairwise comparisons of contributions and earnings in three treatments in order to investigate how the sanction mechanism affects cooperation and punishment behavior under productivity heterogeneity.

Figure 5.1: Group contribution levels as a percentage of optimum (T1)

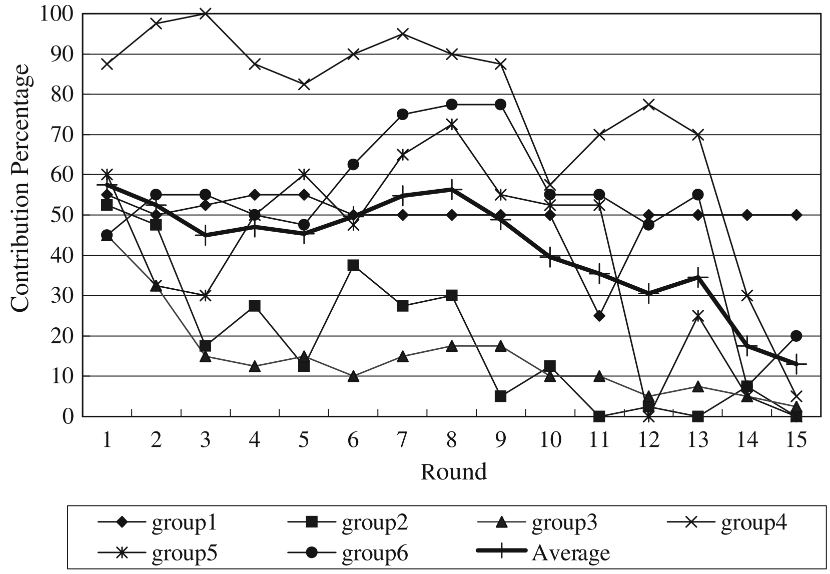


Figure 5.2: Group contribution levels as a percentage of optimum (T2)

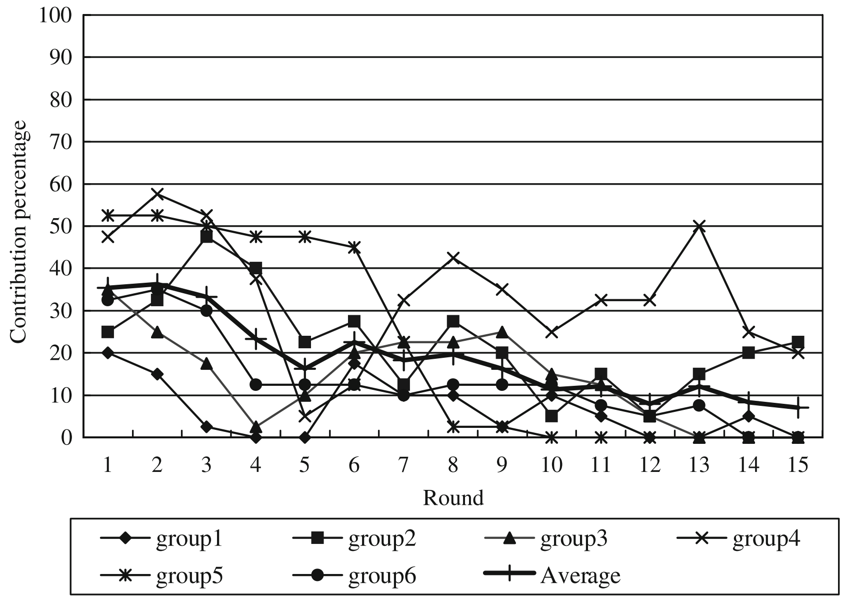


Figure 5.1 and Figure 5.2 display the total contribution levels for each of the six groups as a percentage of the total endowment in T1 and T2, respectively, over the 15 periods. The bold lines indicate the average contributions over all groups within a

treatment. A glance at these lines reveals that both T1 and T2 show patterns similar to those reported in the current literature. The contribution rates are strictly positive at the initial stages (around 55 percent of endowment for T1 and 35 percent of endowment for T2), then fall consistently in later periods to the range between 10 to 20 percent of endowment in both treatments.

Figure 5.3: Average group contribution levels by treatment

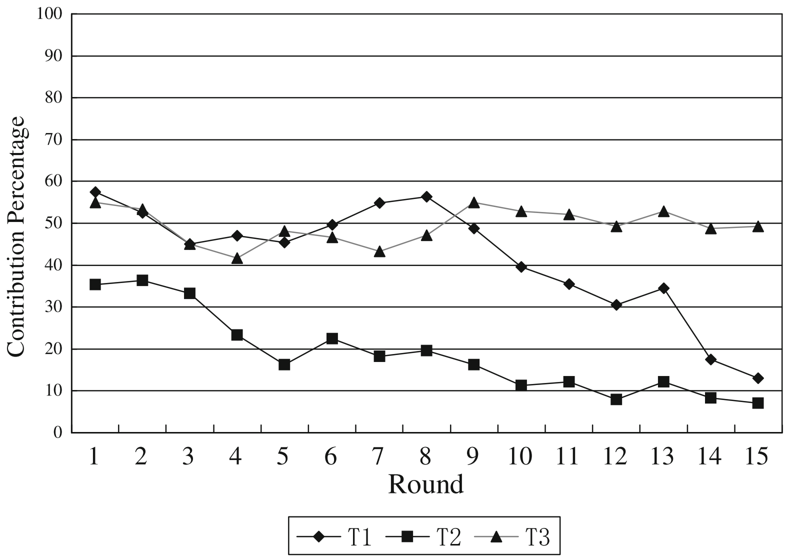


Figure 5.3 puts the average contribution lines by three treatments to the same graph. Note that the line in T2 lies consistently below that in T1 throughout the 15 time periods, indicating that the group average contribution rate in T2 is lower than that in T1.

Result 5.1 *The heterogeneous Productivity Hypothesis (Hypothesis 5.1) is not supported. Without sanctions, the average contribution as a percent of optimum in a heterogeneous group (T2) is less than that of a homogeneous group (T1) with the same average MPCR.*

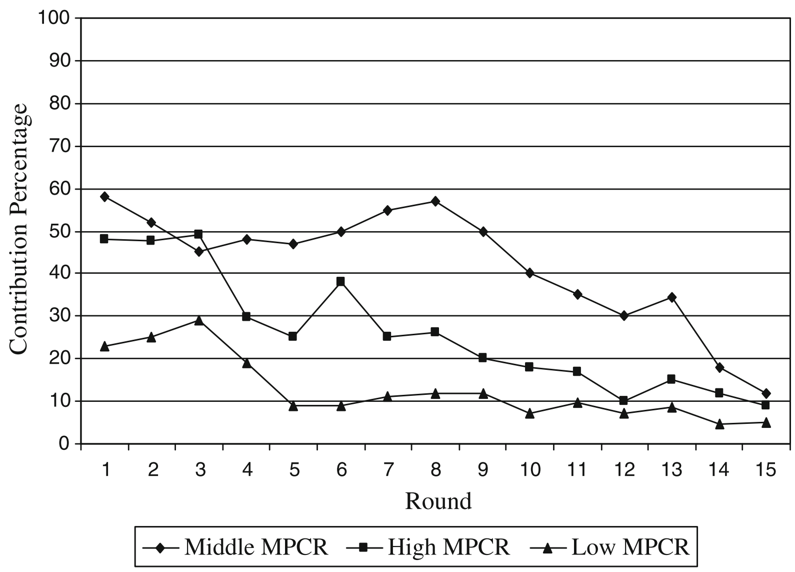
SUPPORT FOR RESULT 5.1:

Group average contribution is 41.8% for T1 and 18.67% for T2. A two-sided Mann-Whitney rank-sum test of difference yields z = -1.922 with $p < 0.1$. Therefore, heterogeneity in productivity seems to reduce the group average contribution level when sanction is not possible.

The contribution rate in T2 is significantly lower than that in T1 because high-productivity subjects (MPCR 0.9) do not significantly increase their contributions, but low-productivity subjects (MPCR 0.3) free ride dramatically, compared to a median productivity group with MPCR 0.6. Figure 5.4 illustrates the mean contributions by period in each of the three MPCR types. Although they vary greatly in the early periods, they all converge to zero in the final periods. The patterns of high-MPCR types and low-MPCR types in this study are consistent with the finding in Fisher et al. (1995). In both studies, contributions of high-MPCR types dominate those of low-MPCR types for all time periods. However, as described in Result 5.2 below, it is not the case that subjects with higher MPCRs will contribute more.

Result 5.2 *The Productivity Determinant Hypothesis (Hypothesis 5.2) is not supported in this study. In particular, the contribution of high-MPCR types is not significantly higher than that of the median-MPCR types in homogeneous groups. Moreover, contribution levels converge to zero for both high- and low-MPCR types in the final periods.*

Figure 5.4: Average contribution levels by MPCR types



SUPPORT FOR RESULT 5.2:

The average contribution level of the low-MPCR types of this study and those of Fisher et al. (1995) are similar in the sense that they both start around 25%, and then decay towards zero over time. The mean contribution for the low-MPCR type is 11.78% of the endowment, which is significantly lower than the other two MPCR types (compared with median productive subjects, a Mann Whitney ranksum test yields z= -2.082 with $p < 0.05$; while compared with highly productive subjects, z = -1.922 with a $p < 0.1$). However, high-MPCR types do not contribute as much as Hypothesis 5.2 leads us to expect if we compare the results of this experiment with those of Fisher et al. (1995). In their study, the average contribution of subjects with an

MPCR of 0.75 is around 40%.[8] If Hypothesis 5.2 holds, then the average contribution of subjects with an MPCR of 0.9 in this study should be higher. However, this figure turns out to be only 25.4%. This number is also lower than that of a homogeneous group with the same average MPCR (41.81%), but the difference is not significant at the 10% level (z = -1.441). As it is shown in Figure 5.4, the contribution levels of median-MPCR types in T1 and high-MPCR types in T2 are quite similar in the first three rounds. In later periods however, contribution level in T2 falls more dramatically than that of T1. Considering that information on productivity, contribution and earnings of all group members is made explicit at the end of each period, it would be natural to link the insufficient contributions observed in high-MPCR types with the severe free riding behavior among the low-MPCR type.

Results 5.1 and 5.2 allow us to conclude that when sanction is not possible, productivity heterogeneity decreases the group cooperation level. We now turn to the situation in which the sanction mechanism is present.

Figure 5.5: Group contribution levels as a percentage of optimum (T3)

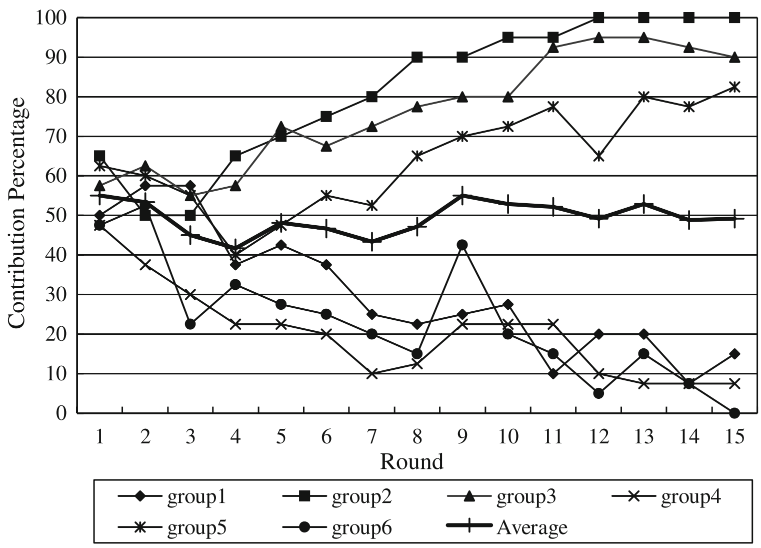


Figure 5.5 illustrates the group average contributions by period in T3. Two extreme cases can be detected from this figure. On one hand, three out of six groups have obvious rising trends in their mean contribution levels from approximately 60% in the first period to nearly 90% in the final period, implying a large extent of cooperation. On the other hand, the other three groups exhibit precisely the opposite behavior. Contribution levels descend from the same starting point of 60% in the first period to less than 10% in the final period, implying a cooperation failure. These two trends

[8]Due to the lack of specific data, this number is estimated based on Figure 5.3 of Fisher et al. (1995).

offset each other, causing the average contribution line in T3 to hover around 50% throughout the time of the study.[9]

Result 5.3 *The monetary Punishment Hypothesis (Hypothesis 5.3) is supported. In the presence of productivity heterogeneity, average contribution levels are higher when sanctions are available. In particular, heterogeneous groups with punishment (T3) have mean contribution rates similar to those of the homogeneous groups without punishment (T1), controlling for the average MPCR.*

SUPPORT FOR RESULT 5.3:

The mean contribution is 18.67% for T2 and 49.33% for T3. The Mann- Whitney rank-sum test of difference between T2 and T3 yields $z = -2.082$, $p < 0.05$. This means that the contribution level of T3 is higher than that of T2 at the 5% significance level. The test statistic comparing T1 with T3 suggests that the difference is not significant at any conventional level ($z = -0.801$).

As indicated in Figure 5.3, the average group contribution level of T3 lies significantly above T2, especially in periods 11-15 ($z=-1.92$ with $p < 0.05$). Comparing T3 to T1, the two lines are entangled in the first nine periods; from the tenth period on, the average contribution level starts to decay steadily in T1, while it remains the same in T3, although the difference is not significant at the 10% level. Consistent with the current literature on punishment (such as Fehr and Gächter (2000)), this result reveals the power of sanction in preventing the decay of cooperation towards the non-cooperative equilibrium level.

The mean contribution of the three groups with rising trends in Figure 5.5 is greater than that of the other three groups with declining trends (A Mann- Whitney rank sum test yields $z = -1.964$ with $p < 0.05$). What is behind such an extreme result - that three groups successfully cooperate while the other three fail? In order to answer this question, I find it helpful to probe sanction behavior by type. Result 5.4 summarizes the findings.

Result 5.4 *The punishment Enforcement Hypothesis (Hypothesis 5.4) is supported in this study. Overall, more punishment points are assigned by high-MPCR types in successful groups than by those in failed groups. Moreover, low-MPCR types in successful groups receive more punishment than their counterparts in the failed groups. In sum, punishment is imposed by high- MPCR subjects on low-MPCR ones in successful groups.*

SUPPORT FOR RESULT 5.4:

Using Figure 5.5, I designate three groups with rising average contribution trends as SG (successful groups); the other three with declining trends are designated as FG (failed groups). Then, I perform pairwise Mann-Whitney rank sum tests of difference

[9]Since there are only six groups in T3, this result may not be robust. Nevertheless, as a tentative study, I treat this fact as given, and proceed to seek a plausible explanation.

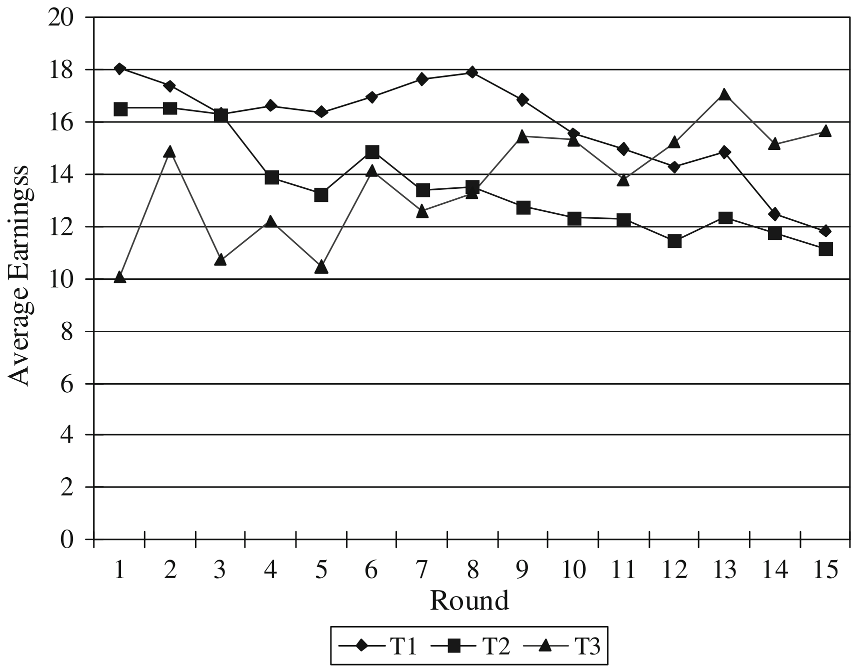
for the punishment points sent and received between two MPCR types in SG and FG for the first five periods.[10] My finding: the high-MPCR types receive the same average points in SG and FG (z= -1.528), but the difference is significant for low-MPCR types (z= -1.964 with $p < 0.05$). In contrast, there is no difference between the average points sent by low-MPCR types in SG and FG (z = -0.655), while for high-MPCR types the difference is significant (z = -1.964 with $p < 0.05$). This indicates that, for those groups that successfully achieve cooperation, punishment is imposed mainly by high-MPCR subjects to low-MPCR subjects.

However, higher contribution levels in T3 due to sanctions do not necessarily imply higher group welfare. The sanctioning system itself is costly to maintain, in that punishment reduces the earnings of both the punisher and the recipient. Even when the sanction mechanism is absent, the lower average contribution rates in T2 do not refer to lower welfare as well. The justification of the statement is that if most of the contributions are from high-productivity subjects in a heterogeneous group, then the high MPCR 0.9 from the group account will compensate for the decrease in the group average contribution level caused by heterogeneity.[11] Figure 5.6 shows the average earnings by period in the three treatments.

[10]The reason for selecting the initial five periods instead of the entire 15 periods is that group average contribution levels are close to each other at this time, and just about to diverge into two trends in these periods. If something happens to cause this divergence, it must be the use of the sanction mechanism. Contrast this to what happens in subsequent periods when players respond to sanctioning behavior, punishment is much less frequently used.

[11]Imagine there is a group of T1 and a group of T2 consisting of four players each. Half of the players in T1 and the two low-MPCR players in T2 do not make any contribution into the group account. Since in T2 it is cheaper for high productivity subjects to produce the same amount of output, in order to yield one token from the group account, the two highMPCR subjects in T2 only need to contribute a total of approximately 1.11 tokens. In T1, however, the other two players with MPCR 0.6 have to contribute a total of approximately 1.67 tokens.

Figure 5.6: Average earnings by treatment



Result 5.5 *The group average earnings of T1, T2 and T3 over 15 periods are similar. Specifically, after the introduction of punishment, the average earnings in T3 are significantly lower than those in T1 in the first five periods. In the final five periods, however, mean earnings in T3 is similar to those in T1 and significantly higher than those in T2.*

SUPPORT FOR RESULT 5.5:

The mean group earnings over 15 periods is 15.85 for T1, and 13.47 for T2, and the former dominates the latter. This difference is insignificant, however, in either the early periods (period 1-5) or the final periods (periods 11-15). This also implies that contributions in T2 are indeed mainly made by high-MPCR subjects. The average earnings of T3 in periods 1-5 is significantly lower than those of T1 ($z = -2.17$, $p < 0.05$), and are similar to those of T2. This implies a substantial decrease in earnings due to sanction costs paid by both punishers and the punished. However, in periods 11-15, the mean earnings of T3 is significantly higher than those of T2 ($z = 2.082$, $p < 0.05$) and the same applies to those of T1. This result suggests that the sanction system effectively counters the increasing free-rider behavior of a standard public good game in the final periods. The contribution increment marks up the loss of punishment, and results in similar average earnings of T3 as those of T1 and T2 over 15 periods ($z = -0.32$ and 1.281, respectively).

5.5.2 Individual analysis

This section mainly concerns the relationship between sanctions and contributions at the individual level in the presence of heterogeneous productivity. Result 5.6 aims at finding the determinants of sanction behavior, and Conjecture 5.1 discusses the effect of sanctions.

Conditional sanction assignment

Fehr and Gächter (2000) observes that agent i assigns more points to k the further k's contribution falls below the group average contribution. Falk et al. (2005) discovers a positive relationship between the number of monetary punishment points that agent i assigns to agent k and the negative deviation of k's contribution from that of i's. Masclet et al. (2003) finds both of the above point-assignment patterns, and also establishes that agents assign more punishments the more recipient's contribution exceeds their own, but fewer points the more the recipient's contribution exceeds the average. This study replicates all of these earlier findings to see whether they also carry over to a group with heterogeneous MPCRs. Besides the above factors, it would be also interesting to see how the MPCR of a punisher and a recipient affect sanction behavior. The regression outcomes of the number of points that agent i assigns to agent k are presented in Result 5.6.

Result 5.6 *The level of monetary sanction that one agent sends to another is increasing in (i) the negative difference of the contribution of the recipient from the average level, and (ii) the negative difference between the contribution of the recipient and the contribution of the punisher, and (iii) when the recipient has a high MPCR of 0.9. The Conditional Punishment Hypothesis (Hypothesis 5.5) is thus supported.*

SUPPORT FOR RESULT 5.6:
Table 5.2 contains the estimated from the following regression model:

$$
\begin{aligned}
P^t_{ik} \quad = \quad & \beta_0 + \beta_1(\max\{0, C^t_i - C^t_k\}) + \beta_2(\max\{0, C^t_k - C^t_i\}) \\
& + \beta_3 \max\{0, \bar{C}^t - C^t_k\} + \beta_4(\max\{0, C^t_k - \bar{C}^t\}) \\
& + \beta_5 \text{type}_i + \beta_6 \text{type}_k + \epsilon^t_{ik} \qquad (5.4) \\
& \text{type}_i = 1 \text{if } i \text{ has MPCR of } 0.9 \\
& \text{type}_k = 1 \text{if } k \text{ has MPCR of } 0.9
\end{aligned}
$$

(5.5)

Because of the large number of zero values for the dependent variable, Tobit and Random Effect Tobit estimations are used for the data. The results are presented in Table 5.2.[12]

[12]The standard errors of the Tobit model are robust to within group correlation. The individual effect in the Random Effect Tobit model is agent i (the punisher).

Table 5.2: Determinants of sanctioning behavior

	Tobit Model (with robust std. errors)	RE Tobit (individual effect: *i*)
β_0 Constant	-5.326***	-6.595***
	(1.975)	(0.750)
β_1 Negative Deviation from i's Own Contribution	0.546***	0.542***
	(0.259)	(0.121)
β_2 Positive Deviation from i's Own Contribution	0.078	0.17
	(0.223)	(0.141)
β_3 Negative Deviation from Average	0.799**	0.848***
	(0.352)	(0.199)
β_4 Positive Deviation from Average	-0.162	-0.296
	(0.242)	(0.241)
β_5 Type i (1 when i has high MPCR)	-0.497	-0.088
	(1.096)	(0.738)
β_6 Type k (1 when k has high MPCR)	0.787*	1.238***
	(0.475)	(0.409)
Log-Likelihood	-744.01	-737.07
Observations	1080	1080

Notes: the number of observations is calculated as: 3(messages) x 4 (players) x 15 (periods) x 6 (groups) = 1080. *** 1% significance level, ** 5% significance level, * 10% significance level. Standard errors in parentheses.

The coefficients of these two models turn out to be rather similar. Both β_1 and β_3 are highly significant, suggesting player i assigns more points to player k the further k's contribution is below his own. On top of this, player i sends even more points if the player k contributes below group average level. These two findings are consistent with current literatures on punishment such as Fehr and Gächter (2000), Masclet et al. (2003), Falk et al. (2005) and Noussair and Tucker (2005). Given all the contribution information in a period, sanction behavior is also sensitive to subjects' types. The positive significant estimated β_6 suggests *ceteris paribus*, a subject with high productivity level will receive more points than a subject with low productivity level. However, the type of the punisher, β_5 is not significant, meaning there is no distinct difference in sanctioning behavior between high and low MPCR types.

Although the estimated β_4 is not significant, the so-called "spiteful behavior" reported by Falk et al. (2005) and Masclet et al. (2003) is unlikely to be the correct explanation in this experiment. Instead, this result may be explained by the asymmetric structure of this game: it is plausible for every member in a group to expect high-MPCR subjects to make more contribution than the low-MPCR subjects would makes, since it would be cheaper. The results in the previous section indicate that the contributions of high MPCR types are significantly higher than those of the low-MPCR types, which implies that it is much more difficult to trigger any possible spiteful preferences in this case than it would be if agents in a group were symmetric. That may be the reason why a higher positive deviation player k's contribution from player i's does not lead to higher punishment from player i.

Sanction effect

One result in Fehr and Gächter (2000) is that agents receiving punishment in time period t usually increase their contributions in the subsequent period. This study replicates the analysis to see whether it still holds.

Conjecture 5.1 [13] *An individual increases his or her contribution level in the subsequent period: (i) the more points he or she receives in period t, and (ii) the further his or her contribution is away from the group average level and (iii) when he or she has an MPCR of 0.9 AND his or her contribution is below the group average level.*

SUPPORT FOR CONJECTURE 5.1:

An estimation of regression contribution change within two time periods on the factors discussed above may be expressed in the following formula. Similar to the Conditional Sanction discussed in the previous section, a dummy variable is also added to the regression representing the MPCR of the recipient.

[13] The reason of naming the relationship as a conjecture is the same as that of Masclet et al. (2003): we cannot be certain that the points themselves instead of other possible variables correlated with the number of pointes received, cause the increase in contribution.

$$\begin{aligned} C_i^{t+1} - C_i^t &= \beta_0 + \beta_1(\sum_k P_{ki}) + \beta_2(C_i^t - \bar{C}^t) \\ &\quad + \beta_3 \text{type}_i + \epsilon_i^t \\ \text{type}_i = 1 \text{if } i \text{ has MPCR of } 0.9 & \end{aligned} \tag{5.6}$$

The coefficient β_1 calibrates the effect of the total number of points player i receives on his change in contribution from one period to the next, is the effect of the difference between individual i's contribution and his group average contribution level in period t, and β_3 is the MPCR type of the punished subject. The model is estimated separately for players who contributed more the group average and less than group average in time period t.[14]

A pooled OLS model and a Random Effect model are performed, and results are highly similar. The coefficients of β_2 in Table 5.3 and Table 5.4 show a significantly negative relation between the deviation from the average and the subsequent change in the contribution level, which is again consistent with the current findings such as Masclet et al. (2003): players have the tendency to make their contributions stay close to the group average level. Furthermore, the coefficient of β_1 in Table 5.3 suggests there is a positive relationship between the points received in period t and the change of contribution when one's contribution is lower than group average level. What is different from Masclet et al. (2003) is that this result carries over to those subjects with contributions higher than group average level. A possible explanation may be again the asymmetric structure of the game, in that high MPCR types are expected to contribute more.

The sign and significance of β_3 in Table 5.3 indicate high MPCR types are more sensitive to punishment, but only when his contribution is below mean level. These players dramatically raise their contribution for a given point they received when their contributions are below group average, but while their contributions exceed group average, the behavioral differences between the two types are not significant any more. This may be due to the insufficient data for low MPCR types for subjects have contributions higher than group average. This also naturally brings to another explanation that it is relatively unusual for low MPCR types to contribute more than group average. But when they do, it may reflect their altruistic characters that are difficult to alter by punishment. As a result, punishments to altruistic low MPCR subject may turn out to be a stimulant for them to contribute even more in the next period.

[14] A Chow test rejects the null hypothesis that there are no behavioral structural differences between players who contribute more than group average and those less than group average. (with $p < 0.01$).

Table 5.3: Determinants of changes in contribution: Low contributors

	OLS	RE
β_0 Constant	-0.705**	-0.567
	(0.292)	(0.391)
β_1 Points received in period t	0.190***	0.221***
	(0.061)	(0.068)
β_2 Deviation from the average	-0.527***	-0.511***
	(0.138)	(0.134)
β_3 (1 when i has high MPCR)	0.895***	0.809*
	(0.317)	(0.478)
R Squared	0.221	0.235
Observations	161	161

Notes: the individual effect in the Random Effect model is player i. *** 1% significance level, ** 5% significance level, * 10% significance level. Standard errors in parentheses.

Table 5.4: Determinants of changes in contribution: High contributors

	OLS	RE
β_0 Constant	-0.272	-0.272
	(0.354)	(0.391)
β_1 Points received in period t	0.266**	0.266**
	(0.110)	-0.110)
β_2 Deviation from the average	-0.731***	-0.731***
	(0.134)	(0.134)
β_3	0.362	0.362
	(0.402)	(0.402)
R Squared	0.178	0.194
Observations	151	151

Notes: the individual effect in the Random Effect model is player i. *** 1% significance level, ** 5% significance level, * 10% significance level. Standard errors in parentheses.

5.6 Conclusion

This study examines cooperation and sanction behavior through a four-person linear public good game in which agents are asymmetric in productivity. The data analysis reveals the following conclusions. Without a sanction mechanism, the mean contribution level of a group with heterogeneous productivity is lower than that of a homogeneous group with the same average MPCR. This is caused by severe free riding among low productivity subjects together with insufficient contribution in high productivity subjects. The imposition of sanction significantly enhances cooperation, but because of the cost of enforcing this system, the earnings under three treatments are similar. In groups which cooperation is successfully achieved, high productivity subjects punish low productivity subjects actively. Conditional on individual contribution levels however, high MPCR types receive more punishment, and behave more responsively by raising higher contributions in the next period.

When the productivity level of every participant is made public, the presumption that high-productivity subjects should carry more responsibility regarding fostering cooperation seems to become common knowledge, or a social norm, in this game.[15] As explained in Fehr and Fischbacher (2004a), this social norm stems from the fact that the behavior of productive agents generates greater side effects than does the behavior of their less productive counterparts. The results mirror the reality in which elites in a society are under higher pressure, since their choices deeply impact a society. The efficiency and robustness of this social norm needs to be further investigated, however, since punishment neither increases welfare nor guarantees a successful cooperation in this asymmetric game. Further research may give agents the power to vote for institutions, in order to investigate whether the "participation constraint" of the sanction mechanism is satisfied, and who would prefer to live in a society with this norm, given the choice. If a public choice process of a social institution is driven by evolutionary forces, this will eventually drive out institution with costly sanctions.

[15]This can be supported from the result that the behavior of high-MPCR subjects influences the cooperation outcome to a large extent. Under sanction institution, it will be those who suffered higher punishment given the same amount of contribution; in successful groups, it will be those who impose severe punishment to defectors; whereas in failed groups, it will be those who do not use sanction system.

5.A Appendix

Experiment Instructions

Presented below are the instructions for the Treatment 3. The instructions for the Treatment 1 and Treatment 2 are identical except for the following differences.

Differences in Treatment 1 instructions and Treatment 3 appearing in this appendix:

1. The whole fourth paragraph of the general instruction (starting with "Before the experiment starts…") is omitted.
2. In the fifth paragraph of the general instruction. The sentence "*Note that each group consists of…*") is omitted.
3. The whole first paragraph in the Detailed Instructions (starting with "Each round consists of…") is omitted.
4. In the income calculation formula under the Subsection of The First Stage, the income from the project is changed into "60 percent of the total number of tokens all 4 group members put into the project".
5. The two examples interpreting the income formula are changed according to the income formula of a homogeneous group with MPCR equals to 0.6.
6. The whole subsection of The Second Stage is omitted.
7. In Screen 1 to Screen 3, all the information about "type" is omitted.
8. Screen 4 and Screen 5 are omitted.
9. In question 1 of the Quiz section, the first questions a) and b) about "types" are omitted.
10. In question 2 to 4 of the Quiz section, the first sentence " You are assigned with…" are omitted.
11. In question 3, the sentence after "You put in 10 tokens to the project" is changed into "All other group members put in 10 tokens to the project", and b) is changed into "The income of the other group members for the period".
12. In question 4, the sentence after "Suppose each group member has an endowment of 10 tokens" is changed into "The other three group members put in a total of 10 tokens to the project".
13. Questions 5 - 8 in the Quiz section are omitted.

Changes in Treatment 2 instructions:

1. The whole first paragraph in the Detailed Instructions (starting with "Each round consists of. . .) is omitted.

2. The whole subsection of The Second Stage is omitted.

3. Screen 4 and Screen 5 are omitted.

4. Questions 5 - 8 in the Quiz section are omitted.

Treatment 3 Instruction
You are now taking part in an economic experiment. If you read the following instructions carefully, you can, depending on your decisions and the decisions of others, earn a considerable amount of money. It is therefore very important that you read these instructions with care.

The instructions we have distributed to you are solely for your private information. **It is prohibited to communicate with the other participants during the experiment.** If you violate this rule, we shall have to exclude you from the experiment and from all payments. Should you have any questions please ask one of us.

During the experiment your entire earnings will be calculated in TOKENS. At the end of the experiment the total amount of tokens you have earned will be converted to euros at the following rate: **25 TOKENS= 1 euro**

Before the experiment starts the computer will assign you with a type. This type can be either "A" or "B". The meaning of type A and type B will be explained in the "Detailed Instructions" below. Your type remains unchanged during the entire experiment.

The experiment is divided into rounds. In each round the participants are divided into groups of four. You will therefore be in a group with 3 other participants. *Note that each group consists of 2 participants with type "A" and 2 participants with type "B".* You will stay in the same group for 15 rounds, but each participant will receive a different identity name, ID 1, 2, 3 or 4 in the group. For example, a participant with ID 1 in this round may not be the same as the participant with ID 1 in another round.

Detailed Instructions:
Each round consists of two stages. In the first stage you have to decide how many tokens you would like to put into to a project. In the second stage you are informed on the inputs of the three other group members to the project. You can decide whether or not to register disapproval of each group member's decision by sending points to them. The following sections describe the activity in detail.

<u>The first stage</u>
At the beginning of each round each participant receives 10 tokens. In the following we call this his or her endowment. Your task is to decide how to use your endowment.

You have to decide how many of the 10 tokens you want to put into a project and how many of them to keep for yourself. Your choice must be an integer, i.e. numbers like 0, 1, 2, . . . and 10.

Your income consists of two parts:
1) The tokens that you have kept for yourself;
2) The income from the project. This equals 90 percent of the total input of group members with type "A" to the project plus 30 percent of the total input of group members with type "B" to the project (including your own input).

Your income in tokens, in first stage is therefore: (10-your input to the project)+ 0.9 × (total input to the project of members with type "A") + 0.3 × (total input to the project of members with type "B")

The income of each group member from the project is calculated in the same way, this means that each group member receives the same income from the project.

For example, suppose the total of the sum of all group members put into the project is 30 tokens. Among these 30 tokens, 18 tokens are put by participants with type "A"; and 12 tokens are put by participants with type "B". In this case each member of the group receives an income from the project of $0.9 \times 18 + 0.3 \times 12 = 19.8$ tokens. If the total sum put into the project is 9 tokens, among which 3 tokens are put by participants with type "A"; and 6 tokens are put by participants with type "B", then each member of the group receives an income of $0.9 \times 3 + 0.\ 3 \times 6 = 4.5$ tokens from the project.

For each token that you keep for yourself, you earn an income of 1 token. For every token you put into the project, the total input rises by one token. If you are type "A", your income from the project would rise by $0.9 \times 1 = 0.9$ token. However the income of the other group members would also raise by 0.9 token each, so that the total income of the group from the project would rise by 3.6 tokens. If you are type "B", your income from the project would rise by $0.3 \times 1 = 0.3$ token. However the income of the other group members would also raise by 0.3 token each, so that the total income of the group from the project would rise by 1.2 tokens. Your input to the project therefore also raises the income of the other group members. On the other hand you earn an income for each token put by the other members to the project. For each token put in by a participant with type "A" you earn $0.9 \times 1 = 0.9$ token; for each token put in by a participant with type "B" you earn $0.3 \times 1 = 0.3$ token.

<u>The second stage</u>
At the beginning of the second stage, a screen will show you how much each of your group members put into the project. In this stage you have the opportunity to register your disapproval of each other group member's decision by assigning points to the

other three participants in your group.

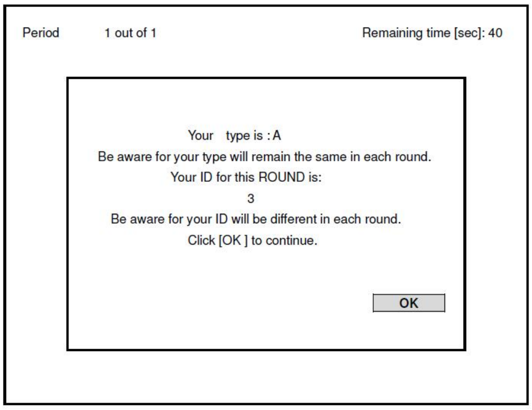
You must decide how many points to send to each of the other three group members. If you do not wish to change the income of a specific group member then you must enter 0. Every point you send will reduce your earnings by 1 token AND reduce the earnings of the participant receiving it by 2 tokens.

Whether and by how much a person's the income from the first stage is reduced depends on the total of the points he/ she received from all of the other members of his/her group. If somebody received a total of 3 points (from all other group members in this period) his or her income would be reduced by 6 tokens. If somebody received a total of 4 points his or her income would be reduced by 8 tokens. The other group members can also assign points to you if they wish to.

We will now explain how the computer screens look like.

SCREEN 1

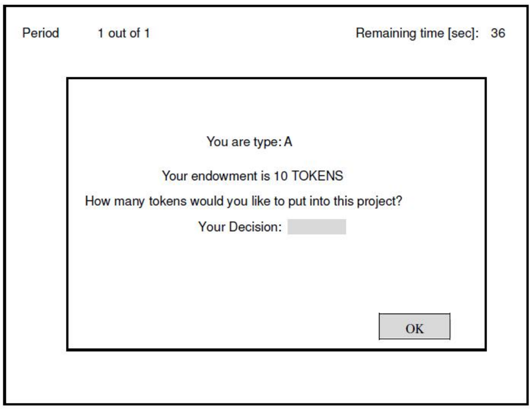
This is the screen that shows your type and your ID for this round. Your type will be either "A" or "B". The ID will range from 1 to 4. After checking your type and ID, click on OK to proceed.



SCREEN 2

Here you decide on how many tokens you will use for the joint project in this round. Use the keyboard to type in one of the numbers 0,1 . . . 10 and confirm your choice by pressing OK.

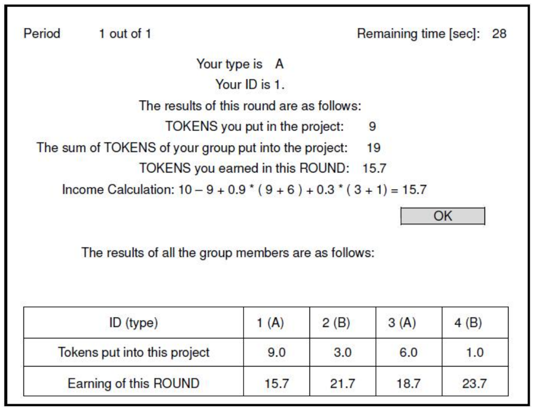
Warning: Before pressing OK, make sure your choice is correct. You cannot change your decision after you have pressed OK.



After having pressed OK, you will be asked to wait until all experiment participants have done the same. The experiment continues only after all experiment participants pressed OK. We therefore kindly ask you not to delay your decision too much. After pressing OK, a waiting screen will appear. After all experiment participants have pressed OK, Screen 3 will appear.

SCREEN 3

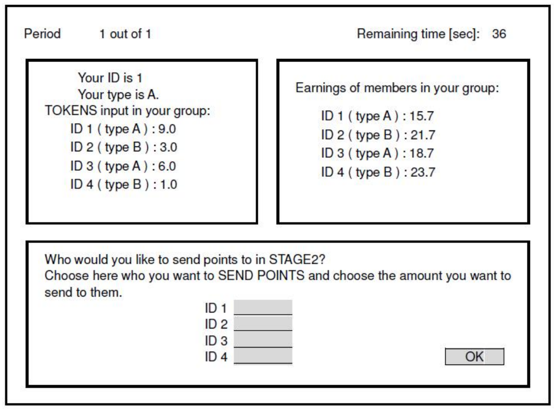
In the upper part of your screen you find a table with information on your type and your ID, the number of tokens chosen by all participants in your group, the income you earned and its calculation. In the lower part, you find a table with information on tokens put into the project and earnings for all group subjects. Click on OK if you are done with checking the information.



SCREEN 4

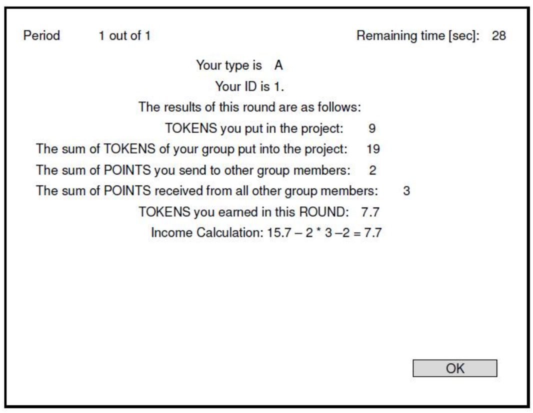
In the upper part of this screen you find a table with information on the type of each participant, the number of tokens chosen for the project by each subject in stage 1 of this round and the number of tokens earned in Stage 1.

In the lower part of this screen, you are asked to make a decision on how many points you would like to assign to reduce earnings of each of the three other participants. Your choice must be integer, i.e. numbers like 0,1,2…10. The sum of points you send to each individual must not exceed your earning in stage 1 this round. Select OK, when you are ready to continue. A waiting screen will appear. The experiment continues only after all participants have pressed OK, and therefore we kindly ask you not to delay your decision too much.



SCREEN 5 In this screen you will be provided with information about this round. You will be shown the tokens you and all participants put into the project, the total number of points you received and assigned to others, the income of this round and its calculation.

Click on OK if you are done with checking the information.



Please raise your hand if you have any questions at this moment.

The experiment now starts with a quiz to make sure that everybody understands how you earn your points. After finishing the quiz, please raise your hand for answer checking. After all participants answered all the questions correctly, the experiment will begin.

Quiz To check your understanding of the experiment, please answer the following questions:

1. About the experiment setting (Yes/ No):
   (a) If you are assigned with type "A", does your type change in different rounds? Yes/No
   (b) Are there 2 participants with type "A" and 2 participants with type "B" in a group? Yes/No
   (c) Are you in the same group in different rounds? Yes/No
   (d) Is the person with ID1 in Round 2 the same with the person with ID1 in Round 3? Yes/No
2. You are assigned with type "A". Suppose each group member has an endowment of 10 tokens. Nobody (including yourself) put in any token to the project. How high is:
   (a) Your income for the period?
   (b) The income of the other group members for the period?
3. You are assigned with type "B". Suppose each group member has an endowment of 10 tokens. You put in 10 tokens to the project. Besides you, a participant with type "A" puts in 3 tokens into the project; another participant with type "A" puts in 6 tokens into the project; and the third participant with type "B" puts in 2 tokens into the project. What is:
   (a) Your income for the period?
   (b) The income of the group member that is type A and put 3 tokens into the project for the period?
4. You are assigned with type "A". Suppose each group member has an endowment of 10 tokens. Besides you, a participant with type "A" puts in 4 tokens into the project; another participant with type "B" puts in 5 tokens into the project; and the third participant with type "B" puts in 3 tokens into the project.
   (a) What is your income if you put in 0 token to the project?
   (b) What is your income if you put in 5 tokens to the project?
5. Suppose in the second stage of a period, you distribute the following amounts of monetary points to the other three group members: 9, 5, and 0. What is the total cost of the tokens you distribute?

6. What are your costs if you send a total of 0 token?
7. By how many tokens will your income from the first stage be reduced, when you receive a total of 0 monetary points from the other group members?
8. By how many tokens will your income from the first stage be reduced, when you receive a total of 4 tokens from the other group members?

CHAPTER 6

VOTING ON PUNISHMENT SYSTEMS WITHIN A HETEROGENEOUS GROUP [1] [2]

This chapter (joint work with Charles Noussair) is published in *Journal of Public Economic Theory* (2011) 13(5): 661-693.

6.1 Introduction

When a group or a society faces a social dilemma, a potential role for an institution to promote or enforce a cooperative norm arises. If such an institutional structure is not imposed exogenously, it must arise endogenously from a social choice process involving the affected individuals. In a situation in which individuals are symmetric and their incentives to cooperate are perfectly aligned, one might argue that agreeing on a mechanism to enforce collective action might be relatively simple. The mechanism can require the individuals concerned to sacrifice an equal amount, all individuals can be punished similarly when deviating from appropriate behavior, and all individuals behaving appropriately can benefit equally.

On the other hand, suppose that players are heterogeneous. Then it is possible that the task of endogenously choosing an appropriate system to promote cooperation may be more difficult, and suboptimal institutions might emerge from the pro-

[1] This chapter is based on Noussair and Tan (2011).

[2] We thank Luc Bissonnette for technical support and CentER at Tilburg University for financial support. We also thank participants at Jinan University, the PET workshop on public economics, the 2008 ESA European meeting, the 2008 NAKE research day, the CESifo Venice Summer Institute Workshop on Behavioral Public Economics, the GSS interdisciplinary workshop at CentER, as well as Wieland Müller, Nikos Nikiforakis, Owen Powell, Louis Putterman, Arno Riedl, Sigrid Suetens, Eric van Damme, Eline van der Heijden, Marie-Claire Villeval, and two anonymous referees for valuable comments.

cess. In this paper, we consider the effect that one particular type of heterogeneity among agents has on the institutions that emerge from a voting process. We employ an experimental approach. Our research strategy is the following. We take a setting, in which it is known from previous experimental results that effective institutions emerge from a simple voting process when individuals are symmetric. We then construct an experimental environment that is identical, except for the fact that there are two types of individual that differ only in the externality generated from their contributions, and introduce an analogous voting process. We find that in the heterogeneous environment, poor institutions often emerge.

The environment that we consider is a version of a popular experimental paradigm to investigate social dilemmas, the voluntary contributions mechanism for public good provision. This is a game, in which players simultaneously choose a fraction of their endowments to contribute toward the provision of a public good. The level of contribution can be readily interpreted as a measure of cooperation. While total group payoff is increasing in the sum of members' contributions, and the social optimum is reached only when all individuals contribute all of their endowments, the dominant strategy for each player is to contribute zero. One focus of research to date has been on the role of decentralized sanctions, the ability of individuals to punish others based on their level of cooperation (e.g. Yamagishi (1986); Ostrom et al. (1992); Fehr and Gächter (2000); Fehr and Gächter (2002); Masclet et al. (2003); Sefton et al. (2007)). Such sanctions have been shown to be effective in increasing cooperation[3], but to have mixed effects on efficiency (Bochet et al. (2006); Cinyabuguma et al. (2006); Nikiforakis and Normann (2008); Tan (2008)), although efficiency increases if the horizon is sufficiently long (Gächter et al. (2008)).

In the studies listed above, the experimenter imposed the sanctioning institution exogenously. There has been recent interest in endogenous punishment institutions that the affected individuals select themselves. Guererk et al. (2005), Guererk et al. (2006) permit individual players to choose, at the beginning of each period, between membership in a group with, and one without, sanctioning opportunities. They find that, while the majority of players opt for the sanction-free institution in the initial periods, the entire population eventually migrates to the group in which sanctioning is permitted. Botelho et al. (2005) construct a 21-period game in which players can vote, by majority rule, whether to allow for punishment in the last period after experiencing both systems with and without sanctioning possibilities for ten periods each. They find a tendency for groups to vote for the system that yielded them a higher

[3]Some limitations apply to this result. If counterpunishment is allowed, much of the beneficial effect is negated (Denant-Boemont et al. (2007); Nikiforakis (2008)). Punishment is also ineffective when the cost of punishment is too high (Nikiforakis and Normann (2008)). There is also some tendency to punish cooperative players. This tendency has been termed anti-social or perverse punishment (Cinyabuguma et al. (2006)), and the incidence of this behavior varies greatly depending on the population studied (Herrmann et al. (2008)).

payoff previously. In their study, this was typically an institution that allowed no punishment. Sutter et al. (2010) let players decide whether to impose a punishment or reward regime at the beginning of a session, by unanimity, and find that individuals prefer rewards, even though payoffs are higher under punishment. Decker et al. (2003) allow individuals to vote for enforcement of the maximum, median, or minimum punishment assigned to an individual, and also report a tendency to vote for the particular institution that yielded the highest payoff previously. They find that the maximum rule is the most effective in generating high contributions. A number of studies find that contribution rates under mechanisms enacted endogenously by group members are higher than when the same institutions are imposed exogenously (Tyran and Feld (2006); Kosfeld et al. (2009); Dal Bo et al. (2010)).[4]

Ertan et al. (2009) is the study most closely related to ours. They study a setting, in which players vote at regular intervals, by majority, on whether to allow punishment of group members who have made contributions that are (a) below-average, (b) above-average, and (c) exactly equal to the average for the group. If a punishment rule is passed, any group member may assign punishment to any individual meeting the criterion of the rule. The rules are not mutually exclusive: any, none, or all of punishment options (a) – (c) could be approved. They observe that most groups, while initially choosing not to allow any punishment at all, eventually vote to allow punishment of below-average contributors exclusively. A minority of groups ban any form of punishment throughout their interaction, and no groups ever vote to allow punishment of above-average contributors. Since both contributions and earnings are highest when individuals can be punished if and only if they contribute less than the group average, the authors conclude that groups successfully converge to the most efficient institutional structure. The focus of our study here is to consider whether this ability of a voting process to converge to the optimal institutional structure is robust to a particular change in the environment. This change is the existence of heterogeneity in the value to the group of individuals' contributions.

In all of the studies mentioned above, agents were homogenous in terms of the value that their contribution generated for the group, so that the tradeoff between the social benefit of cooperation and the private benefit of free riding was identical for each member of the group. In many situations, however, heterogeneity among group members may exist, due to differing productivity of their contributions. Consider, for example, a group of individuals that must complete a project for which all group members will receive equal credit. However, the effort of some group mem-

[4]Two recent studies have the feature that the punishment institution voted into place only governs players who vote in favor of it. In Kroll et al. (2007), agents first play a voluntary contributions game for ten periods, and make and vote on non-binding proposals of minimum total contributions. They report that voting is an empty commitment unless punishment is used to enforce the outcome. Kosfeld et al. (2009) obtain a similar finding, that as long as there is no binding commitment, cooperation is difficult to attain.

bers, because of higher productivity in the required task, yields greater benefits for all than the same effort from other members. For example, one hour of work on the part of one individual may yield the same output as three hours of another individual's work. Because all group members, including the contributor, reap the benefits of an individual's effort, this heterogeneity in productivity is equivalent to a heterogeneous cost of effort among individuals, with those with higher productivity also having lower unit opportunity cost of contribution.[5] Thus, the gains and costs of a contribution depend on who made the contribution. The basic incentive structure of this situation can be captured within the experimental paradigm described above if the marginal per-capita return of a contribution (MPCR) differs depending on who is making the contribution. [6]

Margreiter et al. (2005) is the only study of which we are aware, in which a voting process has been studied in an environment with a type of heterogeneity, similar to that we have studied here. They considered the effect of voting on extraction policies in the context of a common pool resource game, where players are heterogeneous in terms of contribution costs. At the end of every period, players are asked to vote on proposals about the proportion of endowment each group member is to contribute. If a certain proposal is selected by majority vote, it is automatically implemented in the

[5]Heterogeneity in MPCR has been implemented in at least three different ways. One is for every group member to benefit equally from a contribution, but to have the benefit depend on who made the contribution (see for example Tan (2008)). This can be thought of as a situation in which the contributions of some individuals yield a higher return than others, and is the way we implement heterogeneity here. Another way is to have different individuals reap different returns from the same contribution (see for example Reuben and Riedl (2009), Reuben and Riedl (2010)). Yet another is to have the return from the portion of endowment kept and not contributed differ among agents (see Fisher et al. (1995)).

[6]There are a few prior experiments in which MPCR differs among group members. Fisher et al. (1995) conduct a voluntary contributions game in which they assign half of the group members an MPCR of 0.75 and the other half an MPCR of 0.3. By comparing the average group contributions with those of homogenous groups featuring MPCR of 0.75 and 0.3, they find that players with a similar MPCR behave similarly in terms of contributions, regardless of the MPCR of the players with whom they are grouped. Thus, the heterogeneity of MPCR, in itself, does not affect contributions.

Reuben and Riedl (2009) study a privileged group, a setting in which one player has an MPCR of 1.5, and thus a dominant strategy to contribute, and the other players have an MPCR of 0.5. They allow individuals to punish others after observing the contribution profile. They find that punishment is not as effective as in a control group where everyone is endowed with the same MPCR of 0.5. Fewer strong free-riders are punished, and they exhibit a weaker increase in contributions after being punished.

Reuben and Riedl (2010) consider a version of the voluntary contributions game, in which players' initial endowment of income, maximum permissible contribution, and benefit from provision of the public good (the return a player receives from any individual's contribution) differ, depending on the treatment. They include treatments with and without punishment. As in previous studies, they find that punishment increases contributions in all of their treatments. They argue that the norm that is established differs depending on the treatment. In treatments with unequal contribution ceilings, the norm that is enforced is to contribute in proportion to one's maximum possible contribution. In treatments with unequal marginal benefits from public good provision, the norm that seems to be generally enforced is a "proportionality norm", to contribute proportionally to the ratio of the marginal benefits.

next period. The authors find that, compared to homogeneous groups, the number of distinct proposals is markedly larger in heterogeneous groups, but fewer agreements are reached by majority voting. When a proposal is enacted, however, they find that it is generally an efficient one, regardless of whether individuals are homogeneous or heterogeneous.

In this paper, we consider whether two key results of Ertan et al. (2009) apply to a setting in which heterogeneity of group members' productivity, as expressed in the marginal-per-capita return of their contributions, exists. The two results are that (1) permitting punishment but restricting who can receive it to below-average contributors yields the highest payoff among punishment institutions that condition on deviations from average contribution level, and (2) when engaged in repeated opportunities to vote, groups converge to this punishment institution over time. In our experiment, as in Ertan et al., individuals vote at regular intervals on whether players meeting certain criteria are permitted have punishment directed toward them. After a punishment regime is selected, based on majority vote, it is in effect for that group for a fixed and known number of periods. As in the Ertan et al. study, we vary, as a treatment variable, the number of periods that the results of one vote are in effect.

Studying different voting terms is a potentially important aspect of institutional design, and the effect of a punishment system could well depend on the length of time a system is locked in and not subject to change. It is known that the effectiveness of a punishment system may depend on the length of time it is in effect (e.g. Gächter et al. (2008)). A longer horizon means that the stakes for each vote are greater. A longer duration for an inefficient institution might lead to a larger detrimental effect on cooperation, and a good institution can bring greater benefits if it is in effect for a longer period of time. On the other hand, it is possible that more frequent voting can allow a group to experiment more with alternative punishment rules and thereby facilitate convergence to a good institution.

The basic parametric structure of our experimental environment follows Tan (2008). She studies a four-person voluntary contributions game with two types of agent. Two players have an MPCR of 0.9, so that each token they contribute yields 0.9 tokens to all group members, and the other two players have an MPCR of 0.3. All agents are permitted to punish any other agent in any period. Tan finds that punishment is not very effective in increasing contributions among heterogeneous agents. In groups that achieve cooperation, high MPCR players punish low MPCR players frequently if they free ride. However, when controlling for the contribution level of the recipient of punishment, high MPCR players receive more punishment than those with low MPCR.

There is reason to believe that heterogeneity of MPCR may make a difference in which institutions emerge from the voting process. If an institution is enacted to facilitate the enforcement of a norm, the heterogeneous structure of our environment may lead to conflict in the voting process and thus to inefficient outcomes. This may be because there are several plausible norms, specifying differing appropriate levels of contribution for each type of agent (see Reuben and Riedl (2010) for a discussion). This may make it more difficult to achieve consensus on which punishment system to implement and may lead to a conflict between different types of agent. Such conflicts may prove sustained and durable, with adverse long-term effects on contributions and efficiency.

As described in section 6.4, the principal results we obtain are the following. We find that, consistent with Ertan et al. (2009), the most effective institution, in terms of contributions and earnings, is one that allows punishment of below-average contributors only, regardless of productivity type. However, unlike in the Ertan et al. environment, groups often fail to enact this institution, especially when the votes are held relatively frequently. Under these conditions, groups typically establish inefficient regimes, and particularly common is a system in which no punishment is permitted. No group ever votes to enable punishment of all individuals, regardless of their type or contribution level. Players are more likely to vote to allow punishment of below-average contributors and the type other than their own, and they attempt to escape from future penalty exposure by disallowing punishment rules targeting their own type. For many groups, this behavior appears to create an insurmountable roadblock to the establishment of the appropriate institution.

The remainder of the chapter is organized as follows. In Section 6.2, we describe the experiment and in Section 6.3, we advance several hypotheses about the performance of different punishment regimes. In Section 6.4, we present an analysis of the data. Finally, in Section 6.5, we make some concluding remarks.

6.2 The experiment

6.2.1 General setting

The experiment consisted of six sessions that were conducted at CentER Lab, at Tilburg University in the Netherlands. There were two treatments, the *Short-Term* and the *Long-Term* treatments. Each treatment was in effect in three of the sessions. Forty-eight subjects, of whom 42% were females, and all of whom were students at Tilburg University, participated in the study. Some of the subjects had previously participated in economic experiments, but all were inexperienced with the voluntary contributions mechanism. Each subject took part in only one session of the study. On average, a session lasted about 80 minutes (including initial instruction and payment of the subjects), and a subject earned an average of 454 tokens (approximately 18.16 euros). The experiment was programmed and conducted with the z-Tree software (Fischbacher (2007)).

Each session included eight participants that were separated into two groups of four. All individuals remained in the same group for their entire 30-period experimental session, with initial group assignments made randomly. All 30 periods of play counted toward final earnings, and there were no practice periods at the beginning of the sessions. At the beginning of each period, every player was randomly given an identification number from 1 to 4 to distinguish her actions from those of the others during that period. The level of anonymity is thus greater vis-á-vis players of the opposite type. The other player of the same type can identify a player and track his actions over the course of a session. However, players of the other type cannot associate a player with her actions.

Productivity heterogeneity was generated by randomly assigning half of the group members a high MPCR of 0.9 (players of this type will be referred to as type A players) and the other half a low MPCR of 0.3 (type B players). Participants were informed of their type at the beginning of the session, and their types remained fixed for the duration of the session.[7] The instructions used in the experiment were modified on the basis of those used in Ertan et al. (2009) and Tan (2008).

6.2.2 Timing

The 30 periods that made up each session were divided into three segments, as illustrated in Figure 6.1. In the first segment, comprising periods 1 - 3, subjects played the voluntary contributions game without the possibility of punishment. In the second segment, consisting of periods 4 - 6, a second stage was added to the game in which any player could punish any other player, after observing all players' contributions. In the third segment, which made up the remainder of the session (periods 7 -30), the punishment system in place depended on the outcome of a voting process. Voting took place every two periods in the *Short-Term* treatment, and every eight periods in the *Long-Term* treatment. The subjects were not informed about the existence of the next segment of the experiment until the after the previous segment had ended.

In each period of the first segment, the following occurred. Each subject was endowed with ten tokens, with a conversion rate of 25 tokens = 1 Euro. Subjects simultaneously and independently divided their endowment between a private account and a group account. The income of an individual equaled the number of tokens she put in her private account, plus .9 times the total contributions of type A players in her group, plus .3 times the total contribution of type B players in her group. That is, a player's income in each period equaled

$$I_{ij} = 10 - C_{ij} + 0.9 \times \sum_{j=A} C_A + 0.3 \times \sum_{j=B} C_B \tag{6.1}$$

[7]Neutral language was used in the experiment. Players with MPCR of 0.9 were referred to as "type A" and players with MPCR of 0.3 were "type B". Moreover, potentially biased terms such as "contribution" and "punishment" were avoided. For example, punishment was termed as "points that reduce another player's income".

where C_{ij} is the contribution of the *i*th player of type j. This calculation was displayed on subject *i*'s computer screen together with the contributions and earnings of all group members at the end of each period.

In periods 4 - 6, each period was made up of two stages. There was a second, punishment, stage subsequent to the contribution stage described above. In the second stage, subjects were given the opportunity to send a number of points, ranging from 0 to 10, to any other group member. Every point that a particular subject sent to another reduced the sender's earnings by one token and reduced the earnings of the recipient by two tokens. Thus, subject *i*'s income in each period equaled:

$$I_{ij} = 10 - C_{ij} + 0.9 \times \sum_{j=A} C_A + 0.3 \times \sum_{j=B} C_B - \sum_{k \neq i} P_{ik} - 2 \times \sum_{k \neq i} P_{ki} \qquad (6.2)$$

where $\sum_{k \neq i} P_{ik}$ was the sum of points subject i sent to all group members, and $\sum_{k \neq i} P_{ki}$ was the sum of points she received from all others. At the end of each period, the computer displayed the subject's own type, the tokens she and all other group members contributed, the total number of points she received and assigned to others, her income for the current period, and how it was calculated. Subjects were not informed about how much punishment other individuals sent or received.

In the third segment of each session, periods 7 - 30, the following took place. Every two periods in the *Short-Term* treatment, as well as every eight periods in the *Long-Term* treatment, a voting stage occurred at the beginning of a period. During the voting stage, every subject was required to answer each of the following four questions by clicking a box that corresponded to either (a) yes, (b) no, or (c) no preference.[8] The four questions were the following:

I vote to allow a person's earnings to be reduced if the person is a:

1. Type A player assigning less than the average amount to group account.[9]
2. Type A player assigning more than the average amount to group account.
3. Type B player assigning less than the average amount to group account.
4. Type B player assigning more than the average amount to group account.

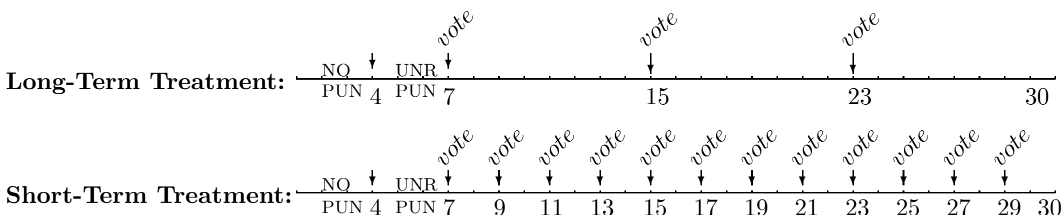
After all subjects gave their answers, the computer tabulated the votes. If the number of "Yes" votes on one of the questions exceeded the number of "No" votes, the

[8] Ertan et al. (2009) also included an option to vote to allow punishment of those players whose contributions were exactly equal to the average. This option is not included in this experiment, however, because if two more questions concerning average contributors of each type are included, the potential number of punishment systems would increase to 64.

[9] In many studies of punishment behavior in the Voluntary Contributions Game, punishment is predominantly directed at those who contribute less than the average (e.g. Fehr and Gächter (2000); Masclet et al. (2003); Denant-Boemont et al. (2007), Tan (2008)) and it is the punishment of low contributors that seems to promote contribution (Ertan et al. (2009)).

reduction specified in the question was allowed; otherwise it was not. A "No preference" vote did not count towards the voting outcome. Since there were four questions, the number of possible outcomes, or punishment institutions, was $2^4 = 16$. Subjects were informed of the punishment system instituted, and the number of periods this institution would be in effect. In the *Long-Term* treatment, a vote occurred every eight periods, and the same institution remained in effect for the eight-period interval following the vote. In the *Short-Term* treatment, a vote took place every two periods, and the resulting system was in effect for the two periods.

Figure 6.1: Timing of activity in each treatment



In every period, regardless of whether a vote occurred in the current period, the contribution and punishment stages occurred in a similar manner as in the second segment. During the punishment stage, subjects decided how many points to send to members meeting the punishment requirement, but were required by the computer program to abide by the restrictions resulting from the last vote, whether it occurred in the current or in a prior period. The feedback presented to subjects at the end of a period in the third segment was the same as in the second segment.

6.2.3 The experiment of Tan (2008)

Tan (2008), in a related study, examines the effect of an exogenously imposed punishment institution on players with heterogeneous productivity. A number of features of that study are similar to the one reported here. The parametric structure of the game is the same in the two studies. Players played the voluntary contributions game under a fixed matching protocol, with two high productivity players with an MPCR of 0.9, and two low productivity players with an MPCR of 0.3. In one treatment, no punishment was possible, as in periods 1 - 3 in the study reported here. In another treatment punishment of any other player was permitted, as in periods 4 - 6 here.

However, there are important differences between the two studies. In the Tan (2008) study, the punishment system is imposed exogenously rather than enacted endogenously by participants themselves. Furthermore, in the Tan experiment, the length of a session is 15 periods, and the same punishment condition remained in effect for the entire session. While it is not the principal purpose of the study reported here, the similar parametric structure between our experiment and Tan (2008) allows us to make rough comparisons between the two studies, and we do so with regard to aspects of individual behavior in section 6.4.

6.3 Hypotheses

Our analysis is organized as a test of several hypotheses. The first two concern whether particular results obtained in Ertan et al. (2009) generalize to our environment. The first hypothesis is that the most effective system for promoting high efficiency is to permit punishment of only below-average contributors, regardless of their productivity, a system we refer to hereafter as *Pun-Low*. The rationale for the hypothesis is that such a system enables the group to punish low contributors to influence their behavior, and prohibits punishment of high contributors in order to encourage them to continue their behavior. *Pun-low* was the most effective of all of the available systems in Ertan et al.'s (2009) environment.

Hypothesis 6.1 *(Efficient Punishment Regime Hypothesis): The punishment regime that yields the greatest efficiency is to allow punishment of below-average contributors only, regardless of productivity (Pun-Low).*

Ertan et al. observed that *Pun-Low* was reached consistently after several iterations of the voting process. We consider whether this finding carries over to our setting with heterogeneous agents. While there is a powerful collective incentive to converge to the most efficient arrangement, there is also reason to believe that it may not do so in an environment with heterogeneous agents. The work of Margreiter et al. (2005) indicates that voting does not guarantee that an institution with high contributions and efficiency emerges when contribution costs vary among group members. Nonetheless, as a null hypothesis we propose that the voting process will behave effectively in discovering the most efficient arrangement.

Hypothesis 6.2 *(Punishment Regime Convergence Hypothesis): Convergence to the most efficient rule occurs over the course of the voting process.*

Note that either Hypotheses 6.1 or 6.2 may be supported while the other one is not supported. *Pun-Low* may lead to the greatest level of efficiency as in Ertan et al. (2009), but may not be attained with the voting process. An institution other than *Pun-Low* may generate the highest efficiency and also be the outcome of the voting process.

The next hypothesis concerns the difference between treatments. A priori, the effect of lengthening the time that an institution is in effect on per-period efficiency is ambiguous, and reflects a tradeoff between the stakes from each vote and experience with the voting process and its results. On one hand, longer governance duration implies that each vote counts more, and these larger stakes may create greater incentives to form more effective institutions. On the other hand, the shorter governance duration in the *Short-Term* treatment offers groups more opportunities to sample and to learn which institutions are relatively effective from experience. Groups also acquire more experience with the voting process itself in the *Short-Term* treatment. Since there are two effects, stakes and experience, operating in opposite directions, we hypothesize that the contributions made and the efficiency attained are not different between the *Short-Term* and the *Long-Term* treatments.

Hypothesis 6.3 *(Governance Duration Hypothesis): Contributions and efficiency are not significantly different between the Short-Term and the Long-Term treatments.*

6.4 Results

The first hypothesis concerns the relative performance of different institutional structures in terms of contributions and efficiency. Table 6.1 displays the average group contributions and earnings under each institution across treatments. The table shows how many times each punishment system was enacted, for how many periods it was in effect, the average contribution and efficiency level (measured as subject earnings) it generated, and its rank among the systems in terms of contribution and efficiency levels. Nine out of 16 possible combinations of punishment rules are enacted at least once in our dataset. The four most common combinations are: (1) to disallow punishment of any agent (which we will refer to as *No-Pun*), (2) to allow punishment of below-average contributors regardless of productivity (*Pun-Low*), (3) to allow punishment of Type B players making below-average contributions (*Pun-B-Low*) and (4) to allow punishment of Type A players making below-average contributions (*Pun-A-Low*). These four structures account for almost 90% of the total voting outcomes. No group ever votes to permit punishment of all agents. Result 6.1 summarizes the main findings concerning the relative performance of the institutions with regard to contributions and efficiency.

Result 6.1 *The efficient punishment regime hypothesis (Hypothesis 6.1) is supported. For the pooled data from both treatments, the most effective regime, in terms of both contributions and earnings, is* Pun-Low, *which allows punishment of players with below-average contributions only, regardless of productivity type.*

SUPPORT: According to Table 6.1, the four most successful institutions all allow punishment of at least some below-average contributors. *Pun-Low* is the most effective institution in terms of contributions in both treatments, and in terms of efficiency in the *Short-Term* treatment. In the *Long-term* treatment, *Pun-Low* is the second-ranked system of efficiency after *Pun-A-Low*. Overall, in *Pun-Low*, the mean contribution level is almost three quarters of the total endowment, which is 73% more than the next best system, *Pun-A-Low*. A Mann-Whitney rank-sum test, using average contributions in each group for the periods that the system is in effect as the unit of observation, indicates that contributions in *Pun-Low* are significantly greater than in *Pun-A-Low* (z = 2.364, p $<$ 0.05) and than in *Pun-B-Low* (z = 2.030, p $<$ 0.05) for the pooled data from both treatments. A similar result holds for efficiency. Although efficiency is not significantly greater in *Pun-Low* compared to *Pun-A-Low* ($z = 0.447$), it is significantly greater than under *Pun-B-Low* ($z = 2.030$, p $<$ 0.05).

If the data from the *Short-Term* treatment is considered separately, contributions in *Pun-Low* are significantly greater than in *Pun-A-Low* at the 10% level ($z = 1.641$) though not greater than under *Pun-B-Low* ($z = 0.479$). *Pun-Low* generates earnings significantly greater than *Pun-A-Low* ($z = 2.236$, $p = 0.036$), as well as borderline significantly greater than *Pun-B-Low*($z = 2.121$, $p = 0.057$). If the data from the *Long-Term* treatment are analyzed separately, pair-wise tests reveal no significant difference between *Pun-Low*, *Pun-A-Low* and *Pun-B-Low* in terms of contributions or group earnings. Contributions in *Pun-Low* are not different from *Pun-A-Low* (z = 1.389) or from *Pun-B-Low*($z = 1.414$). *Pun-Low* generates earnings that are not different from *Pun-A-Low* ($z = .463$) or from *Pun-B-Low* ($z = 1.414$).

There are a number of other interesting patterns evident in the table. *No-Pun* is considerably less effective in generating contributions and earnings than the systems that allow punishment of below-average contributors. There are also some differences in the incidence and relative performance of the institutions between treatments. Institutions permitting punishment of only above-average, but not below-average, contributors appear only in the *Short-Term* treatment. The inefficient *No-Pun* institution is in effect in more than twice as many periods in the *Short-Term* treatment than in the *Long-Term* treatment. The *Pun-A-Low* institution is more effective in the *Long-Term* treatment than in the *Short-Term* treatment both in terms of contribution and earnings, while the opposite holds for *Pun-B-Low*.

Table 6.2 reports the results of a regression estimating the effect of the different institutions on contribution and efficiency levels. The data in the first three periods of the sessions, in which no punishment regime is in effect, are the baseline of the regressions. Unrestricted punishment, in effect in periods 4 - 6 of each session, and in which players can reduce the earnings of any other player, does not lead to higher contribution levels, but does lower earnings, in both treatments. This is indicated by the estimates for β_1.[10] The significantly positive β_2 across all equations confirms the robust effect of allowing for punishment of below-average contributors: this increases group average contribution levels and earnings relative to the baseline. The significantly negative coefficient β_5 in indicates that if players vote out to disallow any form of punishment during the voting stage, group average contributions and earnings

[10] The reason that β_1 is not significant may be the small number of periods during which unrestricted punishment is in effect.

Table 6.1: Frequency and average outcomes of different punishment systems

	Number of Times Enacted	Number of Periods in Effect	Contribution rank	Average Contributions	Efficiency rank	Average Efficiency
***Long-Term* Treatment**						
Pun-Low	8	64	1	6.94	2	18.15
Pun-A-Low	4	32	2	4.13	1	20.22
Pun-B-Low	1	8	4	0.91	5	10.43
No-Pun	3	24	5	0.22	4	10.29
PunAL&PunBH	1	8	3	2.34	3	12.98
PunAH&PunBL	1	8	6	0.28	6	9.66
Pun-B-High	–	–	–	–	–	–
Pun-A-High	–	–	–	–	–	–
Pun-B	–	–	–	–	–	–
Total	18	144				
***Short-Term* Treatment**						
Pun-Low	12	24	1	8.20	1	20.67
Pun-A-Low	9	18	3	3.97	4	13.27
Pun-B-Low	18	36	2	4.57	2	15.21
No-Pun	25	50	7	0.41	7	10.50
PunAL&PunBH	2	4	5	2.88	3	14.40
PunAH&PunBL	1	2	4	3.63	6	11.03
Pun-B-High	1	2	9	0.00	9	10.0
Pun-A-High	3	6	6	2.00	5	12.3
Pun-B	1	2	8	0.13	8	10.03
Total	72	144				
Pooled Data						
Pun-Low	20	88	1	7.28	1	18.84
Pun-A-Low	13	50	2	4.07	2	17.72
Pun-B-Low	19	44	3	3.90	3	14.34
No-Pun	28	74	7	0.35	6	10.43
PunAL&PunBH	3	12	4	2.52	4	13.45
PunAH&PunBL	2	10	6	0.95	9	9.93
Pun-B-High	1	2	9	0.00	8	10.0
Pun-A-High	3	6	5	2.00	5	12.3
Pun-B	1	2	8	0.13	7	10.03
Total	90	288				

Table 6.2: Average group contributions and earnings as a function of punishment system in effect.

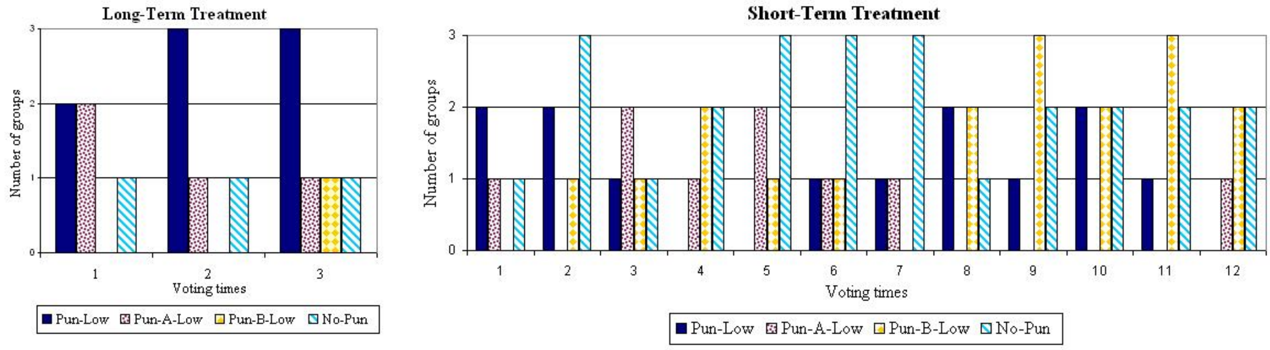
	Average Contributions		Average Earnings	
	Long-Term Treatment	*Short-Term* Treatment	*Long-Term* Treatment	*Short-Term* Treatment
β_1 Unrestricted Punishment	-0.306	0.657	-4.979***	-9.434***
	(0.578)	(0.492)	(1.127)	(2.486)
β_2 *Pun-Low*	2.733***	2.989***	1.978**	5.351**
	(0.497)	(0.502)	(0.969)	(2.317)
β_3 *Pun-A-Low*	-1.780***	-0.535	-1.669	-1.448
	(0.626)	(0.507)	(1.220)	(2.486)
β_4 *Pun-B-Low*	-0.875	1.030**	-2.295	0.496
	(0.826)	(0.451)	(1.615)	(2.134)
β_5 *No Pun*	-1.934**	-1.971***	-2.921*	-4.217**
	(0.843)	(0.417)	(1.642)	(2.025)
β_0 Constant	4.062***	3.459***	16.704***	14.718***
	(0.411)	(0.339)	(0.801)	(1.171)
Adjusted R^2	0.353	0.399	0.273	0.459
Observations	164	166	164	166

Notes: Dependent variable: Group average contributions, $\overline{C_i}$ and group average earnings, $\overline{I_i}$ in period t. *10% significance; **5% significance, ***1% significance. Contribution data corresponding to infrequently enacted institutions such as *PunAL&PunBH*, *PunAH&PunBL*, *PunAH, PunBH and PunB* are excluded because of an insufficient number of observations. The model specification is a fixed effect model with the variable "group" as the individual effect. A Chow test rejects the hypothesis that the coefficients in the *Long-Term* and *Short-Term* treatments are equal for both contributions and earnings. Therefore, we conduct a separate estimation for each treatment.

decrease relative to a situation in which the same system is imposed exogenously.[11]

The second hypothesis concerned whether the most effective institutional structure emerges from the voting process. Our findings are summarized in Result 6.2.

Figure 6.2: Incidence of *Pun-Low*, *Pun-B-Low*, *Pun-A-Low* and *No-Pun* in both treatments



Result 6.2 *The Punishment Regime Convergence Hypothesis (Hypothesis 6.2) is not supported. Institutional rules fail to converge to the efficient* Pun-Low *system in either treatment.*

SUPPORT: Figure 6.2 shows the incidence of each institution in each of the sequence of votes in the two treatments. The horizontal axis of the figures represents the timing of the vote, with voting time "1" indicating the first vote in a session, which occurs at the beginning of period 7. The second vote occurs in period 9 in the *Short-Term* and in period 15 in the *Long-Term* treatment. The vertical axis represents the number of groups, out of a total of six groups, that choose each system. None of the six groups votes for *Pun-Low* during its last vote in the *Short-Term* treatment, while only three of the six groups do so in the *Long-Term* treatment.

As we can see from the data in the figures, the relatively efficient *Pun-Low* institution is chosen with greater frequency in the *Long-Term* treatment. Efficiency refers

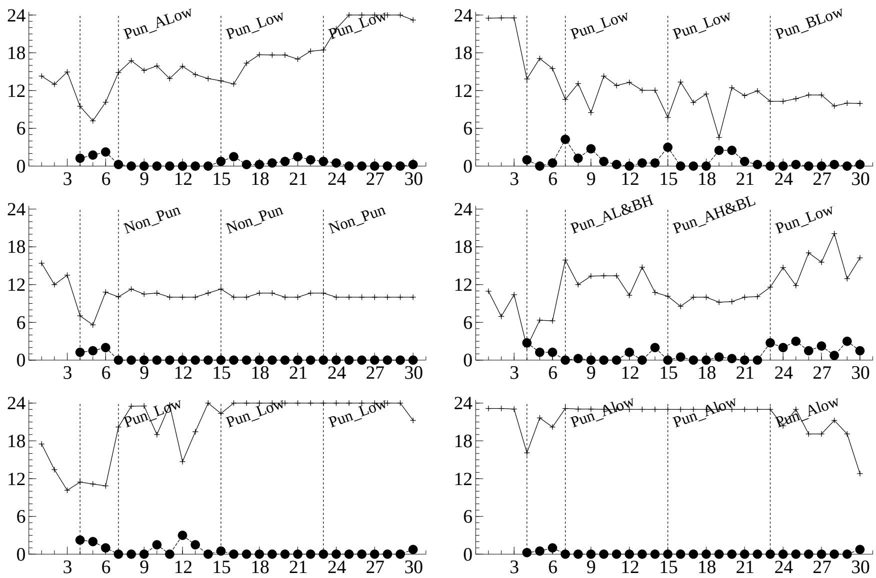
[11]If a time trend is added to all of the regressions in Table 6.2, the results remain substantially the same. In the *Long-Term* treatment, the effect of time on contributions is positive and significant, indicating an upward trend in contributions. In the *Long-Term* treatment, the variables that were previously significant at at least $p < .05$ remain so. The effect of *Pun-B-Low* becomes negatively significant. In the *Short-Term* treatment, the time trend is negative and significant, and the inferences on all other variables remain the same. The effect of time on earnings is positively significant in the *Long-term* treatment. The coefficient of *Pun-Low*, while remaining positive, is no longer significant, when time is included as an independent variable. The coefficients for all of the other treatments become significantly negative. In the *Short-Term* treatment, the effect of time is insignificant and the inclusion of the time trend makes the coefficient on *No Punishment* insignificant at $p = .05$.

to the level of group earnings the institution generates. However, the positive effect on efficiency of the relatively frequent choice of *Pun-Low* in the *Long-Term* treatment is not sufficient to offset the even greater increase in contributions and efficiency that occurs in those instances when subjects in the *Short-Term* treatment select *Pun-Low*. Result 3 summarizes our findings.

Result 6.3 *The Governance Duration Hypothesis (Hypothesis 6.3) cannot be rejected. That is, we cannot reject the null hypothesis that the* Short-Term *and the* Long-Term *treatments generate equal contributions and equal efficiency.*

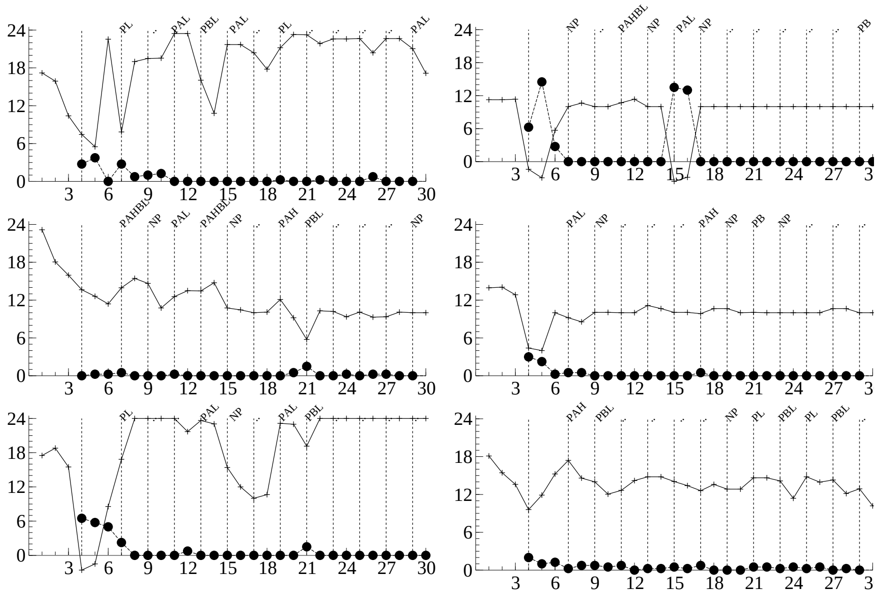
SUPPORT: Mann-Whitney rank sum tests of differences in contributions and efficiency between the *Short-Term* and *Long-Term* treatment in periods 7 to 30, taking each group's activity over those 24 periods as a unit of observation, suggest that neither distributional difference is significant between the two treatments ($p = .749$ for contribution, and $p = .423$ for earnings).

Figure 6.3: Earnings and punishment levels in the *Long-Term* treatment



Notes to Figures 6.3-6.6: Each panel corresponds to one group in the treatment. The horizontal axis designates the number of periods, with the segments indicating the periods in which a specific institution is in effect. The names of the institutions voted into effect are noted in the upper part of each segment. The lines with crosses represent the group average earnings, and the lines with dots represent average number of punishment points.*PL* signifies "allowing punishment of players with below average contributions". *PAL* denotes "allowing punishment of type A players with below average contributions". *PBL* indicates "allowing punishment of type B players with below average contributions". *NP* means "not allowing any form of punishment". *PB* is "allowing punishment of type B players. *PunAHBL* is "allowing punishment of type A with above average contributions or type B players with below average contributions". *PAH* is "allowing punishment of type A players with above average contributions. The sign '..' represents a situation in which the same institution is in effect after the latest vote as after the immediately preceding vote.

Figure 6.4: Earnings and punishment levels in the *Short-Term* treatment

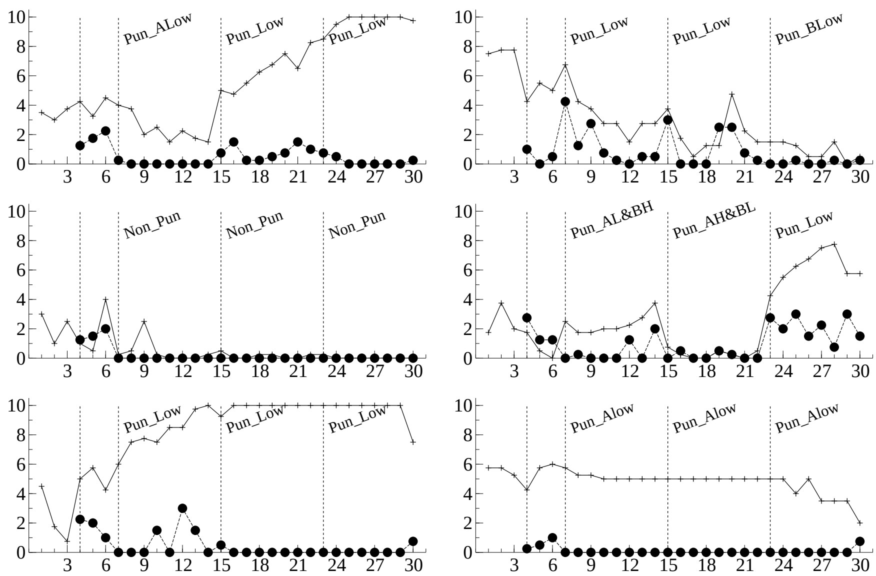


Figures 6.3 and 6.4 show the time series of earnings and punishment points assigned for each group, in the *Long-Term* and *Short-Term* treatments, respectively. The vertical axis indicates the per-capita earnings in tokens (the maximum possible is 24, and the level corresponding to zero contribution and zero punishment is 10), and the number of punishment points allocated per capita. The horizontal axis is the period number. The figures show considerable consistency and thus little variation over time within a group (which complicates statistical inference). When the same institution is in existence, there are also some clear consistencies across groups. [12]

[12]It is reasonable to conjecture that in the experiment, players might prohibit punishment, in order to save time and end the session more quickly. In the experiment, each group could continue at its own pace. However, subjects were aware that they could not receive their payment and leave the laboratory until all players have finished the 30-period session. Therefore, there was little point of hurrying up to finish the experiment early. In the Short-term treatment, each voting round took on average between 9 seconds for the quickest group and 14 for the slowest. The quickest group mainly voted for *Pun-Low* and *Pun-A-Low*. The slowest two groups voted primarily for *No-Pun* and tended to enact relatively uncommon punishment rules such as *punAL&punBH*. The length of a round of contribution averaged between 3 to 5 seconds for each of the six groups. In groups who vote for punishment, it took an average of about 6 extra seconds for the punishment stage (the reason for such short punishment time is that towards the end of the game, when players tend to make high and similar contributions, little punishment occurs). Thus, over a session, a group that prohibited punishment every round would only save 3 minutes (24 periods times 6 seconds for the extra punishment stage) compared to a group that allowed punishment every round. This seems a fairly negligible amount of time saved, which would not affect the play of the game.The differences in overall length between the *Long-Term* and *Short-term* treatments are very small. The average length of play, after the instructions have been read, is 40 minutes in the *Short-Term* compared to 42 minutes in the *Long-Term* treatment. Play in the *Short-Term* treatment

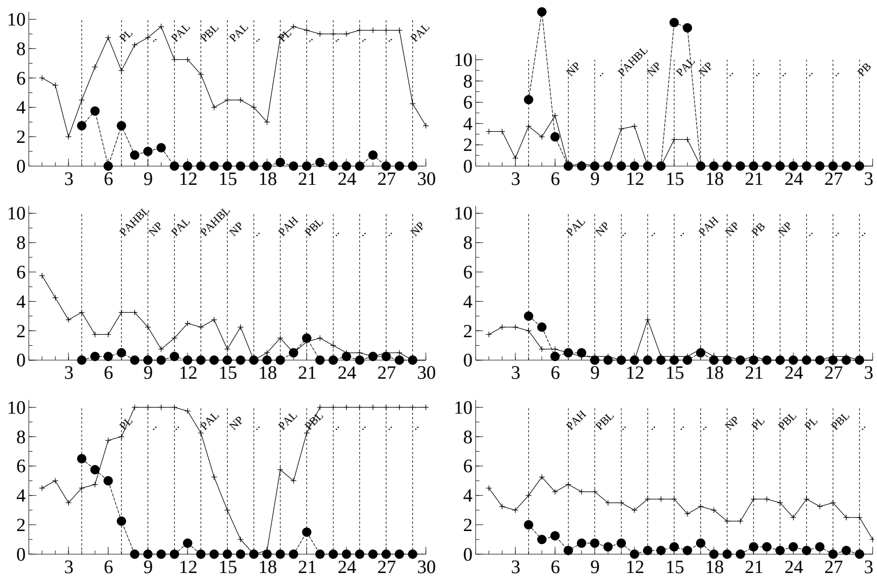
Both figures show that, while *Pun-Low* performs better than the other systems on average in terms of earnings, it only reaches efficiency levels close to the potential maximum in some instances. It is also clear that punishment is effective in raising contributions, at least in the short run; in almost every period after which any punishment points are assigned, there is an increase in group earnings. The *No-Pun* institution consistently leads to zero or near-zero contributions, as reflected in average earnings near ten tokens. In the *Long-Term* treatment, three groups achieve close to the maximum possible level of earnings, and they do so by enacting *Pun-Low* or *Pun-A-Low*. In the *Short-Term* treatment, institutional changes are quite frequent with at least four changes, between one vote and the next, occurring in each group. Only two groups achieve close to maximal earnings by the end of their session. One does so by enacting *Pun-Low*, and the other with *Pun-B-Low*. The figures all show that there is considerable stability within groups, so that most variation is between group rather than within group.

Figure 6.5: Contributions and punishment levels in the *Long-Term* treatment



ranged from 32 to 47 minutes, and play in the Long Term treatment varied from 36 to 45 minutes.

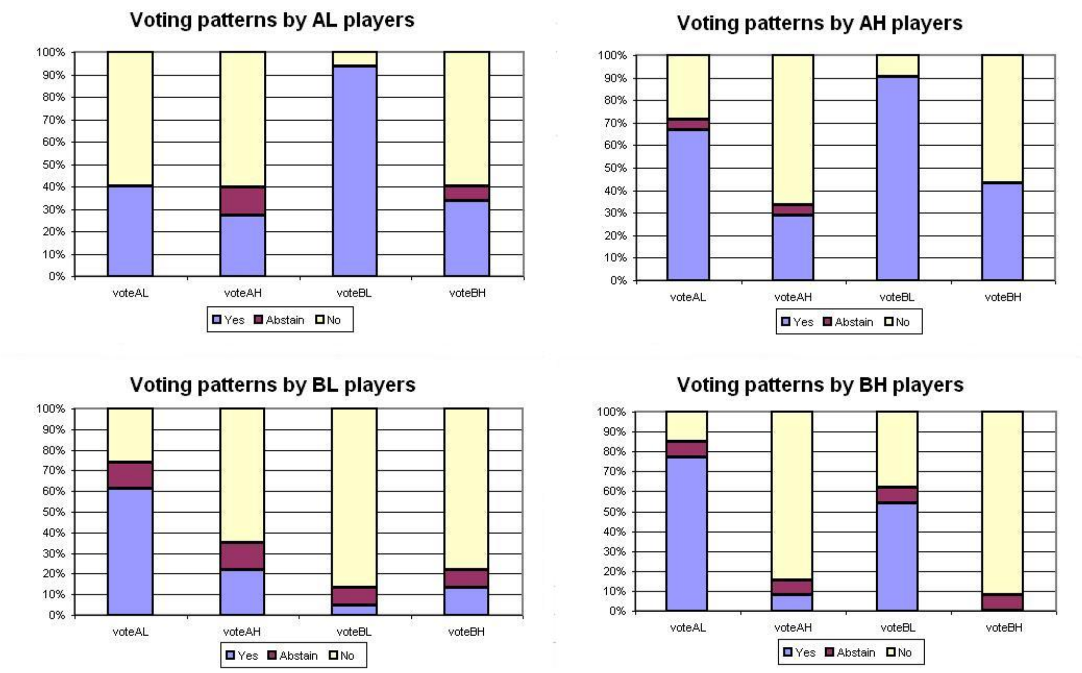
Figure 6.6: Contributions and punishment levels in the *Short-Term* treatment



Figures 6.5 and 6.6 are similar to 6.3 and 6.4, except that they display the patterns and relationships between contributions and punishment. Clear patterns are in evidence. In the *Long-Term* treatment, when *Pun-Low* is in effect, punishment occurs when average contributions are relatively low, and result in an increase in contributions, often to the maximum possible level. When *Pun-A-Low* and *Pun-B-Low* are in force, little punishment is applied and contributions tend to be low relative to under *Pun-Low*. In the *Short-Term* treatment, the connection between punishment and contribution is less evident than in the *Long-Term* treatment, even under *Pun-Low*. The quantity of punishment applied decreases over the first ten periods or so of the sessions, and remains low thereafter, even if contributions remain low.

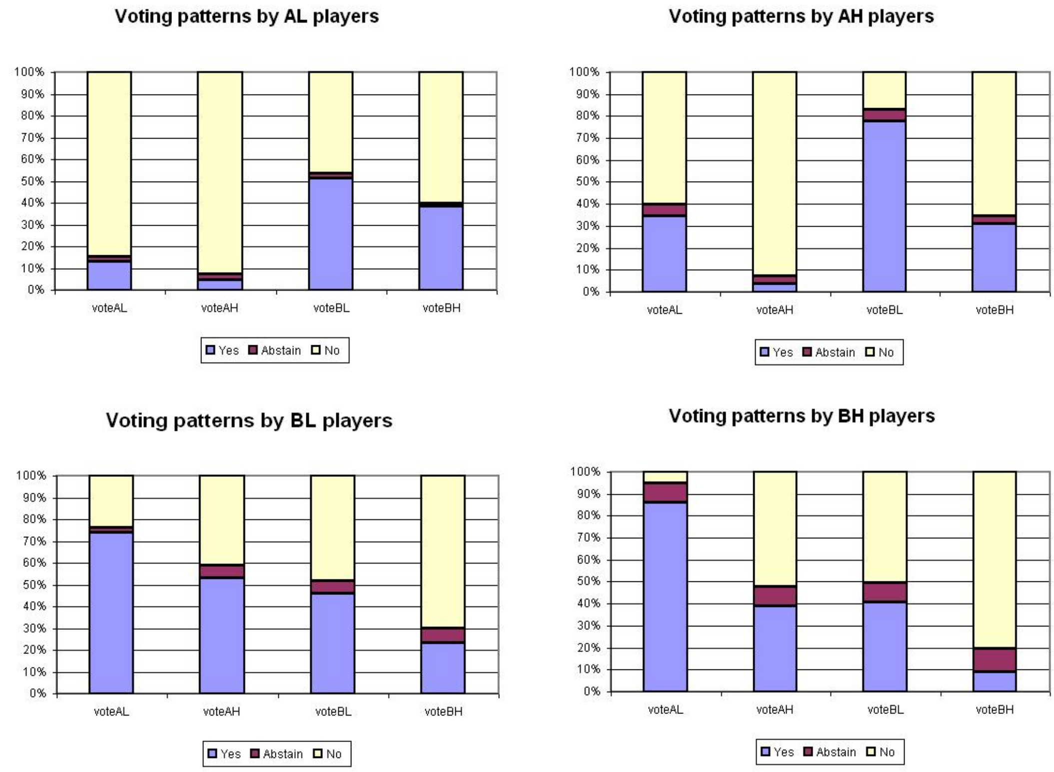
We now turn our focus to how individuals vote, punish, and respond to punishment in our study. Figures 6.7 and 6.8 show the voting behavior of individuals based on their type and contribution level in the period immediately preceding the vote. Each panel in the figures corresponds to the voting behavior of one of the four types/contribution profiles in one of the treatments. Each bar indicates the percentage voting in favor, voting against, and abstaining from each of the four punishment rules. The figures are constructed by classifying each player into one of the four categories: type A below-average contributor (abbreviated to AL), type A above-average contributor (AH), type B below-average contributor (BL) and type B above-average contributor (BH) based on her actual contribution one period before the voting stage. Then the number of “yes”, “no” and “no preference” votes are summed.

Figure 6.7: Voting patterns in the *Long-Term* treatment, percentage of players voting to punish each type and contribution level



Notes to Figures 6.7 and 6.8: "AL player" denotes a Type A player who contributes below the group average, "AH player" is a Type A player who contributes above the average, "BL player" is a Type B player who contributes below the average, and"BH player" is a Type B player who contributes above the average. "VoteAL" means voting in favor of punishing Type A players who contribute less than the average. "VoteAH", "VoteBL", and "VoteBH" are defined analogously.

Figure 6.8: Voting patterns in the *Short-Term* treatment, percentage of players voting to punish each type and contribution level



The figures illustrate the sharp conflicts between above-and below-average contributors, as well as between type A and type B players. When above-average contributors vote in favor of punishment of below-average contributors, they are much more likely to vote in favor of punishment of the other type. Likewise, when they vote against allowing punishment of above-average contributors, they are more likely to vote in favor of banning this punishment for their own type. Below-(above-) average contributors are more willing to vote to allow punishment of above-(below-) average contributors than of players who contribute similarly to themselves.[13]These patterns suggest that players try to shut down punishment channels that may point to them in the future.

Consider the regression reported in Table 6.3. The dependent variable equals 1 if subject i votes to permit a specific punishment rule k in period t, and 0 otherwise.[14] The first six independent variables are dummy variables that equal 1 if the voter or

[13]There is one exception. In the *Long-Term* treatment, AH players, rather than AL players, are more willing to allow for punishment of BH players: 43.5% of AH players vote to allow for punishment of BH players while only 30.8% of AL players vote to allow for punishment of BH players.

[14]There were 20 total abstentions out of 288 submitted votes in the Long-Term treatment, and 53 abstentions out of 1152 submitted votes in the Short-Term treatment. Since these consist of less than 10%

Table 6.3: Voting patterns

	Long-Term Treatment	*Short-Term* Treatment
β_1 Rule targets voter herself	-0.552*	0.148
	(0.327)	(0.096)
β_2 Rule targets opposite type	0.734***	1.148***
	(0.216)	(0.106)
β_3 Rule targets below average contributors	1.171***	0.923***
	(0.253)	(0.294)
β_4 Player i is high MPCR type	0.802***	0.607**
	(0.302)	(0.289)
β_5 Rule targets high MPCR type players	0.198**	-0.119
	(0.097)	(0.103)
β_6 Rule targets opposite contribution behavior	0.170	0.509**
	(0.145)	(0.234)
β_7 Amount of punishment voter received in period t-1	-0.014	-0.054**
	(0.053)	(0.024)
β_8 Amount of punishment voter assigned to others in period t-1	0.048	0.025***
	(0.044)	(0.005)
β_9 Voting round	0.091	0.004
	(0.107)	(0.005)
β_{10} Constant	-1.875***	-1.294***
	(0.328)	(0.363)
Log-Likelihood	-135.29	-582.10
Observations	268	1098

Notes: Dependent variable: Voting by player i in favor of permitting the punishment of player k, $\bar{V}_i^k$. *10% significance; **5% significance, ***1% significance. Only “yes” votes and “no” votes arte included in the estimation; abstentions are excluded. A probit model with observations clustered within group correlation is reported. The results of a logit model are highly similar.

rule satisfies the relevant condition. The seventh and eighth variables are continuous variables representing the total number of punishment points received from other players and sent to other players, respectively, in the period immediately preceding each vote. The variable "Voting round" takes the value 1, 2, or 3 in the *Long-Term* treatment, and 1 to 12 in the *Short-Term* treatment. This variable captures whether a player is less likely to vote to allow punishment as the experiment progresses. Result 4 summarizes the findings.

Result 6.4 *(Voting Behavior) In both treatments, the willingness of players to vote on punishment of a certain player profile is greater (i) if the punishment rule targets the opposite MPCR type, (ii) if the rule targets below-average contributors, and (iii) if the voter has a high MPCR. There is no systematic effect of time on voting behavior. In the* Short-Term *treatment, voting behavior responds to the punishment that the voter assigned and received just prior to the vote.*

SUPPORT: The estimates in Table 6.3 show highly significant positive coefficients of β_2, β_3 and β_4. This indicates that players are more willing to vote in favor of a punishment rule if it targets the opposite productivity type (β_2), below-average contributors (β_3), and if the player voting has a high productivity level (β_4). The variable representing "voting rounds" (β_9) is not significant. In the *Short-Term* treatment, individuals are more likely to vote to punish others, the more punishment they assigned, and the less they received, in the preceding period.

Previous research indicates that the number of punishment points one individual assigns to another is influenced by the difference in contribution between the punishing and the punished agent, as well as the difference between the negative deviations of the recipient's contribution from the group average level (Fehr and Gächter (2000); Masclet et al. (2003); and Falk et al. (2005)). Consider the following regression equation, whose estimates are given in Table 6.4.

$$\begin{aligned} P_{ik}^t &= \beta_0 + \beta_1(\max\{0, C_i^t - C_k^t\}) + \beta_2(\max\{0, C_k^t - C_i^t\}) \\ &\quad + \beta_3 \max\{0, \bar{C}^t - C_k^t\} + \beta_4(\max\{0, C_k^t - \bar{C}^t\}) \\ &\quad + \beta_5 \text{type}_i + \beta_6 \text{type}_k + \epsilon_{ik}^t \end{aligned} \tag{6.3}$$

where type_i if the punisher i has an MPCR of 0.9; type_k if the punished player k has an MPCR of 0.9, and $\bar{C}_t$ is the average contribution within the group in period t. Because of the large number of zero values for the dependent variable, we estimate this

of the total data, we do not include them in our main analysis. Nevertheless, we run Probit regressions with the dependent variable that equals 1 if a subject chooses to abstain and 0 otherwise. All of the independent variables are exactly the same as in Table 3. In the Long-Term treatment, individuals are significantly more likely to abstain if the proposal affects players with the opposite MPCR. In the Short-Term treatment, abstentions are also significantly more likely for a rule targeting type A players.

specification with a Tobit model. The standard errors take within-group correlation into account.

Evidence from prior experimental studies also shows that low contributors on average respond to punishment by raising their contributions in the subsequent period (Fehr and Gächter (2000); Masclet et al. (2003)). The change in the contribution of player i between period t and t+1 can be modeled as:

$$\begin{aligned} C_i^{t+1} - C_i^t &= \beta_0 + \beta_1(\sum_k P_{ki}) + \beta_2(\bar{C}^t - C_i^t) \\ &\quad + \beta_3 \text{type}_i + \beta_4(\text{type}_i \times \sum_k P_{ki}) + \epsilon_i^t \qquad (6.4) \end{aligned}$$

where $\text{type}_i = 1$ if player i is a type A player. β_1 measures the effect of the total number of points subject i receives on her change in contribution from one period to the next if she is of type B, and β_2 is the effect of the difference between individual i's contribution and her group's average contribution level in period t. β_2 would capture a reversion to the mean contribution level, and a change in contribution not due to being punished, such as a fear of subsequent punishment, or an attempt to influence the subsequent institutional choice. β_3 measures any difference in overall contribution change between the two types, and β_4 registers a differential response to punishment on the part of type A or type B players. The estimates of (6.4) for the *Pun-Low* system are given in Table 6.4. The estimates of models (6.3) and (6.4), for the data from the exogenously-imposed unrestricted punishment system studied in Tan (2008), are also included in tables 6.4 and 6.5 under the column labeled *Unrestricted Punishment*. [15] Result 6.5 summarizes the main findings from the estimation of (6.3) and (6.4).

[15]Of course, the coefficients from Tan (2008) have different interpretations, due to the constraints on the punishment that can be applied, the absence of the voting process, and the different histories of contributions and punishment. Apart from an overall reversion to the mean that typically appears in social dilemmas, and a fear of subsequent punishment, agents in the current study may change their contribution level to influence the subsequent institutional choice.

Table 6.4: Determinants of sanctioning behavior under *Pun-Low*

		Pun-Low			
	Unrestricted punishment	Equal contribution norm		Proportional norm	
	(source Tan(2008))	Long-Term	Short-Term	Long-Term	Short-Term
β_0 Constant	-5.326***	3.226***	2.968***	-3.696***	-3.742**
	(1.975)	(0.630)	(0.803)	(0.625)	(1.486)
β_1 Recipient k's Negative Deviation from i's Contribution	0.546**	0.752***	1.769***	0.563***	0.717***
$(\max\{0, c_i - c_k\})$	(0.259)	(0.130)	(0.671)	(0.121)	(0.151)
β_2 Recipient k's Positive Deviation from i's Contribution	0.078	1.144***	–	1.253***	–
$(\max\{0, c_k - c_i\})$	(0.223)	(0.423)	–	(0.435)	–
β_3 Recipient k's Negative Deviation from Average	0.799**	-0.201	-1.346	-0.176	-0.224
$(\max\{0, \bar{c} - c_k\})$	(0.352)	(0.199)	(0.862)	(0.125)	(0.274)
β_4 Recipient k's Positive Deviation from Average	-0.162	–	–	–	–
$(\max\{0, c_k - \bar{c}\})$	(0.242)	–	–	–	–
$\beta_5$type$_i$	-0.497	-0.325	0.431	0.627	2.105
(= 1 if i is Type A)	(1.096)	(0.529)	(0.673)	(0.768)	(2.352)
$\beta_6$type$_k$	0.787*	-0.268	0.084	-0.304	0.115
(= 1 if k is Type A)	(0.475)	(0.533)	(0.669)	(0.532)	(0.657)
Log-Likelihood	-744.01	-277.957	-75.205	-277.479	-76.232
Observation	1080	278	99	278	99

Notes: Dependent Variable: Punishment points player i sends to player k at time t: P^t_{ik}. *10% significance; **5% significance, ***1% significance. A Tobit model is used with standard errors robust to within group correlation. Since the earnings of above-average contributors are not allowed to be reduced, the β_4 coefficient is not included in the *Pun-Low* estimation.

Table 6.5: Changes in contributions of below-average contributors as a function of punishment received and type

	Unrestricted Punishment (source Tan (2008))	Equal contribution norm *Pun-Low* Pooled Treatments & Types	*Low* MPCR Type Only	*High* MPCR Type Only	Proportional norm *Pun-Low* Pooled Treatments & Types	*Low* MPCR Type Only	*High* MPCR Type Only
β_1 Punishment Received at Period t	0.289***	0.185	-0.045	0.734***	0.299	0.226	0.689***
	(0.067)	(0.162)	(0.175)	(0.234)	(0.201)	(0.191)	(0.200)
β_2 Deviation from average	0.169***	0.512**	1.075***	-0.276	0.242	0.425**	-0.237
	(0.056)	(0.205)	(0.242)	(0.354)	(0.208)	(0.202)	(0.519)
β_3 Type i	0.058	0.314	–	–	-1.284	–	–
	(0.401)	(0.805)	–	–	(1.327)	–	–
β_4 Punishment Received $\times$ Type i	0.340***	0.304	–	–	0.307	–	–
	(0.127)	(0.202)	–	–	(0.263)	–	–
β_0 Constant	0.891***	-0.886	-1.225*	-0.092	0.006	0.448	0.739
	(0.258)	(0.603)	(0.726)	(0.380)	(0.84)	(0.781)	(1.619)
Adjusted R squared	0.25	0.324	0.401	0.584	0.323	0.213	0.579
Observations	161	66	40	26	66	40	26

Notes: Dependent variable: changes of contribution $C_i^{t+1} - C_i^t$. *10% significance; **5% significance, ***1% significance. The model specification procedure is as follows. Firstly, for *Pun-Low* institution, a Chow test cannot reject the null hypothesis that the contribution responses of the *Long-Term* treatment and *Short-Term* treatment are identical ($F(3,58) = 0.93$, $p = 0.432$). Therefore we combine the two treatments in the estimation. We then compare a pooled OLS with robust standard errors; a fixed effect model and a random effect model. The pooled OLS proves to be the best specification in a Lagrange-Multiplier test compared with the random effect model and in an F-test compared with the fixed effects model.

Result 6.5 *(Punishment Behavior and Responses) Under* Pun-Low *the severity of punishment is increasing in the negative difference between the contributions of the recipient and the punisher. Type B players increase their contributions more in the subsequent period, the farther their contribution is below the group average. Type A players increase their contributions in response to punishment.*

SUPPORT: The estimates in Table 6.4 show that in both *Pun-Low* and under the unrestricted punishment regime, there is a positive relation between the punishment points player i sends to player k and the extent to which player k's contribution was below that of player i's. Unlike under unrestricted punishment, there is no relationship between the type of the sanctioned party, and punishment behavior. Table 6.5 indicates that in the *Pun-Low* regime, the contribution level increases significantly, the more a player's contribution is below group average (β_2).The insignificance of the β_1 coefficient suggests that it is not the actual sanction, but rather the possibility of punishment, which triggers increases in contribution for type B players when punishment of below-average contributors in enabled. The significant β_4 coefficient in the Unrestricted Punishment data indicates that type A players are more likely to increase their contribution in response to punishment than type B players. Under *Pun-Low*, type A players respond to punishment by increasing their contribution while type B players do not. On the other hand, Type Bs tend to increase their contribution in the direction of the average when not punished, while the type As do not exhibit this tendency.[16]

Reuben and Riedl (2010) document the enforcement of a norm that prescribes contributions that are proportional to the ratio of private marginal benefits, which is plausible in our setting. Self-serving biases or strategic motives can lead individuals to punish those who deviate from norms that favor the sanctioner, more than those who deviate from other potential norms. The lack of consensus about a norm translates into conflict about which punishment institution to vote for. We rerun regression (6.3) and (6.4) by replacing group average contributions with contributions proportional to the MPCR types of the players.[17] More specifically, we assume a norm that high MPCR players contribute three times as much as the low MPCR players. The regression results in the last two columns of Table 6.4 and Table 6.5 indicate that the findings presented in Result 6.5 remain robust. Moreover, the proportional norm is not necessary a better specification than the equal contribution norm, as the fitness of the models do not increase significantly.

[16] Inclusion of a time trend in the regressions reported in Table 6.4 yields a significantly negative effect of time. However, the significance of other variables remains exactly the same. For Table 6.5, when a time trend is included, the regression results remain the same and the coefficient of the time variable is not significant.

[17] There are other reasonable norms, such as every player setting contribution levels to attempt to equalize earnings. However, replacing contributions with earnings field the exact same result in this setting. As every player benefits exactly the same from the public goods, any differences in earnings between two players boil down to differences in their contributions.

6.5 Discussion and conclusion

We have studied the voting behavior of groups that face a social dilemma. At regular intervals, the groups vote to select a punishment institution, a set of conditions under which individuals may punish others. The game we study is one in which interacting agents make voting, contribution and punishment decisions over a series of periods. The voting decision, when combined with the votes of others, can influence subsequent contribution decisions and constrain punishments. In turn, the prior history of group play, as well as beliefs about future play, can influence contribution, punishment, and voting decisions. The issue we investigate is whether the most efficient institution, in terms of yielding maximal gains to the group, emerges from the voting process. We pose this question for an environment, in which players are heterogeneous in terms of the benefit that their contributions yield to the group.

It is clear which institutions promote high levels of contributions and efficiency. These are institutions that allow punishment of low contributors only. In particular, we observe that *Pun-Low*, which allows punishment of low contributors regardless of how much surplus their contribution would have created, while immunizing high contributors, performs well in generating high average contributions and efficiency levels. *Pun-Low* exploits the willingness of individuals to punish low contributors, and the tendency for the punished low contributors to cooperate more in response. It does so while eliminating the adverse consequences of the punishment of high contributors, which are a reduced level of cooperation on the part of punished individuals, and a resulting decrease in earnings. We thus extend a previous result obtained by Ertan et al. (2009) in a similar setting with symmetric players, to an environment with asymmetric players. When the *Pun-Low* system is in place, little punishment is actually applied. The threat of punishment is typically sufficient to generate high levels of cooperation at a low cost of enforcement.

However, we find that groups often fail to adopt this institution even after having repeated opportunities to vote for its enactment. The heterogeneity of players, and the ability to vote to selectively punish individuals by type as well as by behavior, appears to lead to negative consequences. There are several possible explanations for the difference in the effectiveness of the voting process between our setting, and one in which players are symmetric. The explanations are not mutually exclusive. One possibility is that the purpose of punishment becomes ambiguous because the motivation to free-ride becomes opaque. Fuster and Meier (2010) document such an effect in another setting. They find that in a treatment where there are private incentives to contribute, free riders are punished less harshly, and hence cooperation is not as common as in a treatment without such incentives. A possible explanation is that the private reward received by high contributors reduces their anger at free-riders, and thus reduces their tendency to punish them. Here, the voting outcome might be interpreted as granting license to individuals to free-ride or an obligation to cooperate. Moreover, free riding might be viewed as reciprocation for others' voting behavior.

Another plausible explanation for the poor choice of institution is that the designation of two different player types creates a common identity, which leads to solidarity between players of the same type. Unfortunately, our design cannot offer us direct evidence for or against this common identity argument. In order to identify and isolate a possible identity effect of this nature, one could include another treatment in which the MPCRs are the same for all group members, but some individuals are designated as "Type A", and others as "Type B" players.

However, we believe that the feature that generates the difference in outcomes between our setting and the homogeneous one is the pervasiveness of defensive voting. Conflicts are generated as players attempt to prevent punishment that can be directed at themselves, while also seeking to enable punishment of players who differ in both contribution behavior and productivity type. The result is that, because majority support is required to enact a punishment rule, groups often find themselves with no ability to punish some or all free riders, and thus without a mechanism for enforcing high contributions. Prohibiting punishment of one type seems to be often interpreted by that type as a license to free ride. [18]

There are obvious limitations to our study. Within our design, it is not possible to evaluate how effective a punishment regime is in a heterogeneous group compared with a homogeneous group. This is because our design does not include a baseline where players are homogeneous. We can only rely on a comparison between the qualitative patterns we have observed and those of Ertan et al. (2009). We thus must operate under the auxiliary hypothesis that any differences between our and their subjects and procedures do not overwhelm or interact with differences in the environment in such a way as to negate the main conclusions. Moreover, we have only allowed a limited selection of the possible punishment rules. In particular, the rules that we have considered treat both types in the same manner. It is plausible that a rule that allows the punishment of those who contribute below the average of their respective type would be better received by all group members and would allow heterogeneous groups to function as well as homogenous groups. Another open question is whether homogeneous groups would still converge to efficient outcomes if people could vote to allow the punishment of all low contributors excluding themselves. This would allow us to directly address the question of whether it is really player asymmetry that drives the inability to enact efficient institutions or rather, whether it is the specific menu of policies that we have studied.

[18]For instance, when *Pun-A-Low* is in effect in the *Long-Term* treatment, both type B players in a group contribute less than the group average in 90.63% of periods, and one of the two type B players contributes less than the average in all of the remaining periods. In contrast, under *Pun-B-Low*, both type B players contribute less than the average of their group in 50% of periods.

6.A Appendix

Experiment instructions

Presented below are the instructions for the 3-Vote Treatment. The instruction for the 12-Vote Treatment is identical except only one sentence in Part III. In the sentence "After the voting, the decision is in effect for eight rounds. Then you will be asked to vote again for every eight roundsé"Ÿï¿½the number of rounds is changed from an "eight" to a "two".

The *Long-Term* treatment instruction

EXPERIMENT INSTRUCTIONS (PART I)

You are now taking part in an economic experiment. If you read the following instructions carefully, you can, depending on your decisions and the decisions of others, earn a considerable amount of money. It is therefore very important that you read these instructions with care.

The instructions we have distributed to you are solely for your private information. It is prohibited to communicate with the other participants during the experiment. Should you have any questions please ask us. If you violate this rule, we shall have to exclude you from the experiment and from all payments. During the experiment your entire earnings will be calculated in TOKENS. At the end of the experiment the total number of tokens you have earned will be converted to euros at the following rate: **25 TOKENS= 1 euro**.

Before the experiment starts the computer will assign you with a type. This type can be either "A" or "B". The meaning of type A and type B will be explained in the "etailed Instructions" below. Your type remains unchanged during the entire experiment. The experiment is divided into rounds. In each round the participants are divided into groups of four. You will therefore be in a group with 3 other participants. Note that each group consists of 2 participants with type "A" and 2 participants with type "B". You will stay in the same group for 30 rounds, but each participant will receive a different identity name, ID 1, 2, 3 or 4 within the group in each round. For example, a participant with ID 1 in this round may not be the same as a participant with ID 1 in another round.

Detailed Instructions:

At the beginning of each round each participant receives 10 tokens. In the following we call this his or her endowment. Your task is to decide how to use your endowment. You have to decide how many of the 10 tokens you want to put into a project and how many of them to keep for yourself. Your choice should be an integer, i.e.numbers such as 0, 1, 2,. . . ,10.

Your income consists of two parts: 1) the tokens which you have kept for yourself; 2) the income from the project. This equals 90 percent of the total input of group members with type "A" to the project plus 30 percent of the total input of group members with type "B" to the project (including your own input).

Your income in tokens in each round is therefore:
(10-your input to the project)
+ 0.9$\times$ (total input to the project of members with type "A")
+ 0.3$\times$ (total input to the project of members with type "B")

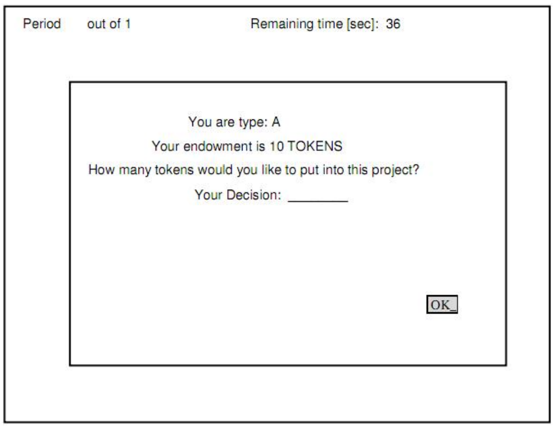
The income of each group member from the project is calculated in the same way, this means that each group member receives the same income from the project.

For example, suppose the total sum of all group members put into the project is 30 tokens. Among these 30 tokens, 18 tokens are put by participants with type "A"; and 12 tokens are put by participants with type "B". In this case each member of the group receives an income from the project of $0.9 \times 18 + 0.3 \times 12 = 19.8$ tokens. If the total sum put into the project is 9 tokens, among which 3 tokens are put by participants with type "A"; and 6 tokens are put by participants with type "B", then each member of the group receives an income of $0.9 \times 3 + 0.3 \times 6 = 4.5$ tokens from the project.

For each token that you keep for yourself you earn an income of 1 token. For every token you put into the project instead, the total input rises by one token. If you are type "A", your income from the project would rise by $0.9 \times 1 = 0.9$ tokens. However the income of the other group members would also increase by 0.9 tokens each, so that the total income of the group from the project would rise by 3.6 tokens. If you are type "B", your income from the project would rise by $0.3 \times 1 = 0.3$ tokens. However the income of the other group members would also increase by 0.3 tokens each, so that the total income of the group from the project would rise by 1.2 tokens. Your input to the project therefore also raises the income of the other group members. On the other hand you earn an income for each token put by the other members to the project. For each token put in by a participant with type "A" you earn $0.9 \times 1 = 0.9$ tokens; for each token put in by a participant with "B" type you earn $0.3 \times 1 = 0.3$ tokens.

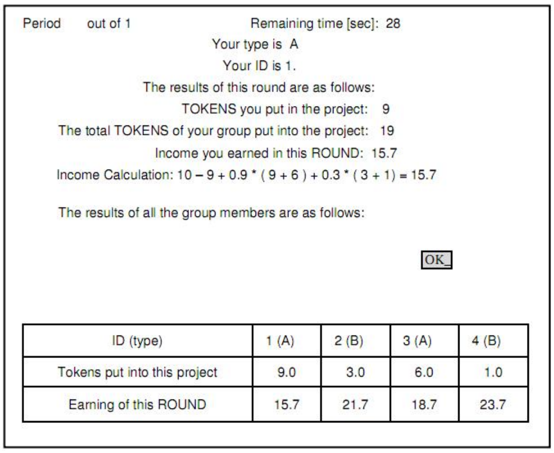
We will now explain how the computer screens look like.

SCREEN 1
This is the screen which shows your type and your ID for this round. The ID will range from 1 to 4. After checking this information, click on OK to proceed.



SCREEN 2

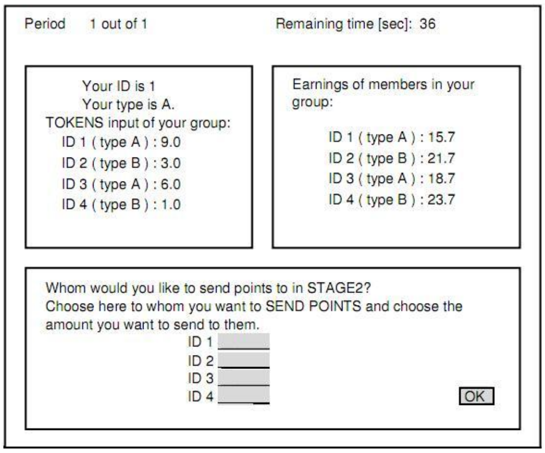
Here you decide on how many tokens you will use for the project in this round. Use the keyboard to type in one of the numbers 0,1,...,10 and confirm your choice by pressing OK.



Warning: Before pressing OK, make sure your choice is correct. You cannot change your decision after you have pressed OK. After having pressed OK, you will be asked to wait until all experiment participants have done the same. The experiment continues only after all experiment participants have pressed OK. We therefore kindly ask you not to delay your decision too much. After pressing OK, a waiting screen will appear. After all experiment participants have pressed OK, Screen 3 will appear.

SCREEN 3

In the upper part of your screen you find a table with information on your type and your ID, the number of tokens chosen by all participants in your group, the income you earned and its calculation. In the lower part, you find a table with information on tokens put into the project and earnings for all group subjects.



Click on OK if you are done with checking the information.

The experiment will begin with three rounds of play. Each round you begin with a new 10 tokens to allocate, and each round's earnings are independent of the others. After these three rounds, there will be further instructions.

Please raise your hand if you have any questions at this moment.

The experiment now starts with a quiz to make sure that everybody understands how you earn your points. After finishing the quiz, please raise your hand for answer checking. After all participants answered all the questions correctly, the experiment will begin.

Quiz

To check your understanding of the experiment, please answer the following questions:

About the experiment setting (Yes/ No):

1. If you are assigned with type “A”, does your type change in different rounds? Yes/No
2. Are there 2 participants with type “A” and 2 participants with type “B” in a group? Yes/No
3. Are you in the same group in different rounds? Yes/No
4. Is a person with ID1 in Round 2 definitely the same with a person with ID1 in Round 3? Yes/No

2. You are assigned with type “A”. Suppose each group member has an endowment of 10 tokens. Nobody (including yourself) put in any tokens to the project. How high is:

1. Your income for the period?
2. The income of the other group members for the period?

3. You are assigned with type “B”. Suppose each group member has an endowment of 10 tokens. You put in 10 tokens to the project. Besides you, a participant with type “A” puts in 3 tokens into the project; another participant with type “A” puts in 6 tokens into the project; and the third participant with type “B” puts in 2 tokens into the project . What is:

1. Your income for the period?
2. The income of the group member which is type A and put 3 tokens into the project for the period?

4. You are assigned with type “A”. Suppose each group member has an endowment of 10 tokens. Besides you, a participant with type “A” puts in 4 tokens into the project; another participant with type “B” puts in 5 tokens into the project; and the third participant with type “B” puts in 3 tokens into the project .

1. What is your income if you put in 0 tokens to the project?
2. What is your income if you put in 5 tokens to the project?

EXPERIMENT INSTRUCTIONS (PART II)

After this break for instructions, you and the same three members of your group will be interacting for another three rounds. As with the three rounds just completed, each of these rounds begins with a decision on assigning ten tokens to a group account or to a personal account. This time, however, each round also includes a second stage of decision-making.

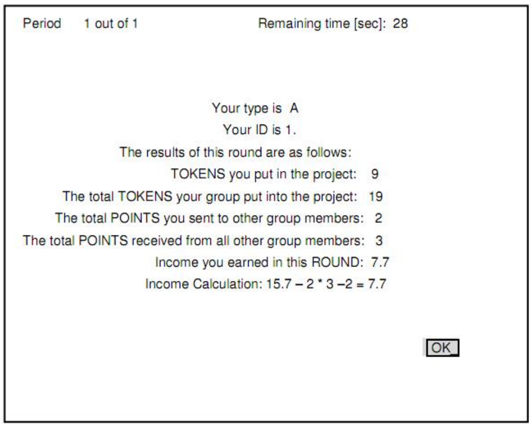
At the beginning of the second stage, a screen will show you how much each of your group members puts into the project. In this stage you have the opportunity to register your disapproval of each other group member's decision by assigning points to the other three participants in your group. You must decide how many points to send to each of the other three group members. If you do not wish to change the income of a specific group member then you must enter 0. Every point you send will reduce your earnings by 1 token AND reduce the earnings of the participant receiving it by 2 tokens.

Whether and by how much a person's income from the first stage is reduced depends on the total of the points he/ she received from all of the other members of his/her group. If somebody received a total of 3 points (from all other group members in this round), his or her income would be reduced by 6 tokens. If somebody received a total of 4 points, his or her income would be reduced by 8 tokens. The other group members can also assign points to you if they wish to.

Your total income from this round (two stages together) is therefore calculated as follows:
= (income from the 1st stage
+ points assigned to other participants)
- 2× total points received by three other participants.

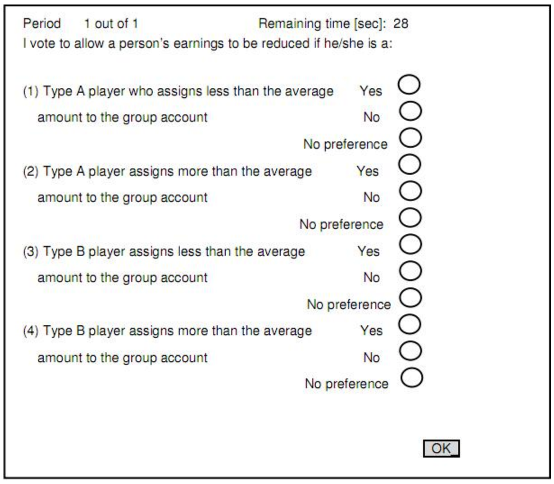
We will now explain how the computer screens look like. Note that Screen 1 to Screen 3 are exactly the same as the first three rounds.

SCREEN 4



In the upper part of this screen you find a table with information on the type of each participant, the number of tokens chosen for the project by each subject in stage 1 of this round and the number of tokens earned in Stage 1. In the lower part of this screen, you are asked to make a decision on how many points you would like to assign to reduce earnings of each of the three other participants. Your choice must be integer, i.e. numbers like 0,1,2,. . . ,10. Select OK, when you are ready to continue. A waiting screen will appear. The experiment continues only after all participants have pressed OK, and therefore we kindly ask you not to delay your decision too much.

SCREEN 5



In this screen you will be provided with information about this round. You will be shown the tokens you and all participants put into the project, the total number of points you received and assigned to others, the income of this round and its calculation. Click on OK if you are done with checking the information.

The experiment will continue with another three rounds of play. After these three rounds, there will be further instructions.

Please raise your hand if you have any question at the moment.

The experiment now starts with a quiz to make sure that everybody understands how you earn your points. After finishing the quiz, please raise your hand for answer checking. After all participants answered all the questions correctly, the experiment will continue.

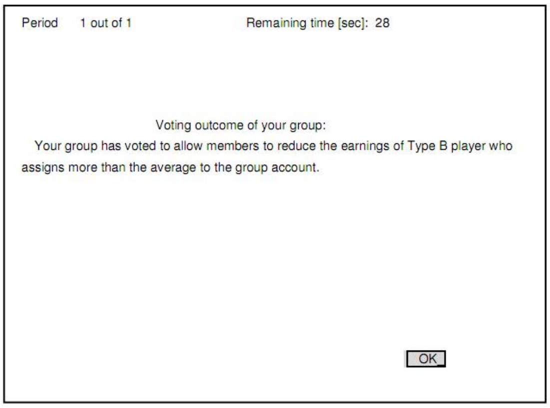
1. Suppose in the second stage of a period, you distribute the following amounts of monetary points to the other three group members: 9, 5, and 0.
   (a) What is the total cost of the tokens you distribute?
   (b) What are your costs if you send a total of 0 tokens?
2. By how many tokens will your income from the first stage be reduced, when you receive a total of 0 monetary points from the other group members?
3. By how many tokens will your income from the first stage be reduced, when you receive a total of 5 tokens from the other group members?

EXPERIMENT INSTRUCTIONS (PART III)

In the remaining parts of the experiment, you will play for twelve sets of two rounds each in the same group of four subjects. Before this part begins, each group will decide, by voting, whether to permit subjects to reduce one another's earnings after learning of their assignments to the group account. It will be possible to allow reductions of a type A or type B subject who assigns more than the average to the group account, and/or of type A or type B subjects who assigns less than the average to the group account. Once the decision has been made by your group, it will be in force for the next two rounds of the experiment.

We will now explain how the computer screens look like.

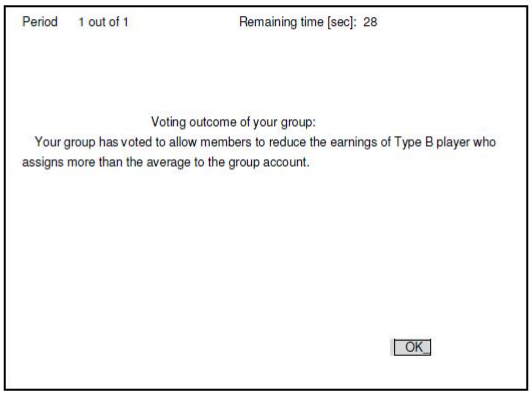
SCREEN 6



In this screen you are asked to answer "Yes", "No", or "No preference" to four questions by clicking the box to the right of each of the three choices. For each question, if the number of "Yes" vote in your group exceeds the number of "No" vote, the reductions in question will be allowed; otherwise they will not. A "No preference" vote does not count towards the voting outcome. Click on OK if you are done with answering the questions.

Warning: Before pressing OK, make sure your choice is correct. You cannot change your decision after you have pressed OK. After having pressed OK, you will be asked to wait until all experiment participants have done the same. The experiment continues only after all experiment participants pressed OK. We therefore kindly ask you not to delay your decision too much. After pressing OK, a waiting screen will appear. After all experiment participants have pressed OK, Screen 7 will appear.

SCREEN 7



In this screen you will be informed of the outcome under which your group will operate for the next two rounds. The possible messages are listed in the appendix below. Note that only one of these messages will show up on the screen.

Click on OK if you are done with checking the information.

After the voting, the decision is in effect for eight rounds. Then you will be asked to vote again for every eight rounds. During the reduction stage of each round, if the earnings of certain group members are voted "allow to be reduced" because of the rules decided by your group, you can decide whether to send points to the group members meeting the description. On the other hand, the reduction boxes for any individuals whom your group has decided cannot have their earnings reduced will automatically appear with zeros inside, which cannot be changed.

It is important that you fully understand the voting process before we continue. Please raise your hand if you have any questions at this moment. If not, the experiment will continue.

Possible messages:

1. Your group has voted not to allow group members to reduce one another's earnings.
2. Your group has voted to allow members to reduce the earnings of any other group member.
3. Your group has voted to allow members to reduce the earnings of Type B players assigning less than the average to the group account.
4. Your group has voted to allow members to reduce the earnings of Type B players assigning more than the average to the group account.
5. Your group has voted to allow members to reduce the earnings of Type A players assigning less than the average to the group account.
6. Your group has voted to allow members to reduce the earnings of Type A players assigning more than the average to the group account.
7. Your group has voted to allow group members to reduce the earnings of players assigning less than average to the group account regardless of their types.
8. Your group has voted to allow group members to reduce the earnings of players assigning more than average to the group account regardless of their types.
9. Your group has voted to allow group members to reduce the earnings of Type B players.
10. Your group has voted to allow group members to reduce the earnings of Type B players assigning less than average AND Type A players assigning more than average to the group account.
11. Your group has voted to allow group members to reduce the earnings of Type B players assigning more than average AND Type A players assigning less than average to the group account.
12. Your group has voted to allow group members to reduce the earnings of Type A players.
13. Your group has voted to allow group members to reduce the earnings of Type B players AND Type A players assigning less than average to the group account.
14. Your group has voted to allow group members to reduce the earnings of Type B players AND Type A players assigning more than average to the group account.
15. Your group has voted to allow group members to reduce the earnings of Type A players AND Type B players assigning more than average to the group account.
16. Your group has voted to allow group members to reduce the earnings of Type A players AND Type B players assigning less than average to the group account.

CHAPTER 7

CONCLUSION

This dissertation investigates and compares several economic institutions (Chapter 2 and 3) as well as the impact of heterogeneity on institution performances (Chapter 4 and 5) and selection (Chapter 6).

A general conclusion of Chapter 2 and 3 is that institutions do shape behavior. Institutions matter since their changes affect information states and individual incentives. In Chapter 2, for instance, the data from the experiment indicate that focusing on auditing effort (i.e. how many audits will be conducted) instead of auditing probability increases the cost-effectiveness of an auditing mechanism. The emphasis on effort introduces strategic uncertainty which does not exist in the traditional rule. The bounded rule associates the audit probability of a player with the actions of others. As many subjects are strategic uncertainty averse, they shy away from this situation since they are not sure how others are going to react. Hence, future mechanism design can make better use of the typical patterns established in boundedly-rational human beings to increase efficiency.

Chapter 3 implies that emotion can be a very powerful driving force for the usage of punishment mechanisms in social dilemmas. Hence, even though a third-party approval mechanism is useful in turning down punishment targeting cooperators, it dampens the power of punishment towards defectors. One implication is that when anti-social punishment is not pervasive, the intervention of a third party reduces the effectiveness of the peer punishment mechanism in promoting cooperation. The results also shed light on the importance of designing effective punishment institutions to enforce cooperation when the proposal rights are separated from the enforcement right of punishment.

The second part of my dissertation shows that heterogeneity among players influences the well-established properties of economic institutions. In particular, I examine two types of heterogeneity: making decisions alone or in a group (Chapter 4) and the cost of contributions in public goods (Chapter 5). Chapter 4 shows that the established notion "groups are more rational and behave closer to the sub-game perfect equilibrium" does not carry over to repeated sequential games. The reason is that groups learn to collude more than individuals over the course of the game. So far, most of the theory is silent on the behavioral difference between groups and individuals, or in some cases, states explicitly that they are not supposed to make any difference. The results of Chapter 4 call upon the need to incorporate some of these findings into future theoretical studies.

In the public goods provision study explored in Chapter 5, asymmetric cost and benefits from the public goods result in different cooperation incentives among players. Consequently, low-incentive players heavily free ride on the high-incentive ones. As many people are conditional cooperators, the free-riding behavior is perceived highly negatively, melting down cooperation toward the end of the game. The implication is that it is important to incentivize players with lower incentives to contribute, for their free-riding decisions have a negative spill-over effect on the others. A potential mechanism to tackle this problem is to collect players' contributions sequentially. Considering 1) most people are conditional cooperators and 2) the marginal contribution benefit/cost varies among individuals, the contribution should be elicited firstly from the group of individuals who have the highest benefit/lowest cost to contribute, and then from the remaining low-incentive contributors. This solution echoes the act of publicly announcing big donors who help to provide public facilities (e.g. schools, theatres, or green areas in the city), or matching small donors with big donors with seed money (see, for example, List and Lucking-Reiley (2002)).

Results from Chapter 6 show that suboptimal institutions arise when individuals have the opportunity to endogenously select institutions to govern themselves. The most likely reason lies in the self-serving bias among subjects. They turn down punishment channels that might target themselves in the short-run future, even though punishing low contributors results in long-run benefits. Since many groups got stuck in inefficient institutions, a possible solution is to let groups learn from each other. For instance, an extra information feedback stage could be imposed before the institution selection stage. This information can help groups learn and emulate the successful examples (i.e. choosing institutions only targeting low contributors).

Considering homogeneity across individuals is more an exception than the norm in a society, future research should further explore the impact of heterogeneity on the performance and selection of economic institutions. As people differ in many aspects (gender, ability, endowment, etc), an interesting question to explore is how these social characteristics link with each other, and whether they affect decisions in different environments in a systematic manner. For instance, people who are more pro-social/ reciprocal might tend to be more affected by information from others' choices in market situations. Moreover, more research is needed to understand the behavioral norm across heterogeneous individuals, as lacking consensus on an appropriate behavioral norm often leads to conflicting interests. Only based upon these findings can future institutional designs better serve different agents and improve efficiency.

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